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Edward Jay Epstein

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How Many Games in Town? —the Pros and Cons of Legalized Gambling
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Methadone: the forlorn hope

EDWARD JAY EPSTEIN

IN 1898, the Bayer Company of Germany synthesized a white crystallized compound from morphine and marketed it under the trade name of Heroin. The new drug, though three times as powerful a pain-reliever as morphine, was purported to be non-addictive, and was even recommended in authoritative medical journals as a means of treating addiction to morphine and other drugs. Less than 10 years later, the medical profession, confronted with an increasing number of heroin addicts, recognized heroin as a highly addictive and dangerous drug, and by the 1920's the United States government had effectively outlawed heroin. Medical opinion remained divided, however, on the question of how to treat the existing heroin addicts. Some doctors believed that heroin induced a permanent change in the bio-chemistry of its victims, and therefore addicts would have to be maintained on heroin for the rest of their lives. Other doctors held to the theory that heroin itself was the toxic agent in the body, and therefore addicts could be successfully "detoxified" by withdrawing the drug from them.

Although this controversy was never really settled on the basis of medical evidence, the American Medical Association strongly endorsed the view that the only proper treatment for heroin addiction was totally to withdraw the addict from the use of the drug. In 1923,

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with the full support of the medical establishment, the federal government closed the heroin clinics (which had been set up to maintain addicts on a minimum dosage of heroin after the Harrison Narcotics Control Act of 1914 and subsequent court decisions had illegalized their normal sources of supply) and relentlessly prosecuted doctors who disbursed heroin to their patients for the purpose of maintaining their habit. Up until the late 1960's, detoxification remained the only legal remedy for addiction, and some 5,000 doctors were jailed for dispensing opiates in violation of this policy.

The development of methadone

Methadone was developed by German scientists during World War II as a substitute for heroin and morphine that could be manufactured in domestic laboratories. After the War, the formula for this synthetic drug was given to American drug companies. It was manufactured for "investigative use" by Eli Lilly & Company, who described it as a "synthetic narcotic analgesic with multiple actions quantitatively similar to those of morphine" and "a narcotic with significant potential for abuse with dependence-producing characteristics."

Though somewhat less powerful than heroin, methadone produces virtually the same analgesic and sedative effects, and is no less addictive than natural opiates. Because of its similarity to heroin, methadone initially was used in hospitals for "detoxifying" addicts—a procedure in which addicts are given progressively decreased dosages of a narcotic until they are "drug-free." As a detoxifying agent, methadone has distinct advantages over the natural opiates: It is less expensive than heroin and morphine (which have to be imported into the United States); it can be administered orally rather than by intravenous injection; and since its effects last for up to 24 hours, it need not be given at such frequent intervals.

In the 1960's, however, methadone was proposed for a very different form of treatment: maintenance. Reviving the half-century old controversy, a group of doctors proposed that heroin addicts, rather than being detoxified, should be maintained on a relatively high dosage of methadone dispensed by government-approved clinics. This time the maintenance view prevailed, and by 1973 some 73,000 addicts in the United States were receiving daily dosages of a synthetic opiate. Underlying this radical change in government policy—and in medical perceptions—was the powerful hope that the new drug would not only undercut heroin addiction, but also alleviate the problems of urban crime and unemployment in America.

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The "Cinderella drug"

In 1964, under the auspices of the New York Health Research Council, Dr. Vincent P. Dole, a research associate at the Rockefeller Institute, and Dr. Marie E. Nyswander, a psychiatrist, initiated a series of experiments which laid the conceptual groundwork for methadone maintenance. In the original experiment, 22 heroin addicts were hospitalized and were given progressively increased dosages of methadone until they were "stabilized" on it—a state which is achieved when the patient has neither withdrawal symptoms nor a craving for increased dosages of methadone. Once "stabilized," the patients left the hospital and lived at home, returning once a day for their oral dosage of methadone and periodic tests for other drugs and side-effects. (Although drug maintenance technically violated the regulations of the Federal Bureau of Narcotics, the new director of the Bureau decided not to intervene on the grounds that the Dole-Nyswander project was "bona fide" research.) In August 1965, Doctors Dole and Nyswander issued a "progress report" which revealed that "patients who before treatment appeared hopelessly addicted are now engaged in useful occupations and are not using diacetylmorphine (heroin). As measured by social performance, these patients have ceased to be addicts."[1]* Such remarkable results were possible, Dole and Nyswander suggested, because methadone established "a pharmacological block" against the effects of heroin, and therefore protected the patients "against readdiction."[2] While the mechanics of the "blockade" were not then clearly defined, the inference was that methadone somehow prevented heroin from producing its usual effects. Establishing this "blockade" required a daily dosage of from 80 to 150 mgs. of methadone, the dosage previously given to terminal cancer patients.

On the basis of such encouraging findings, the project was vastly expanded, and the following year (August 1966) Dole and Nyswander reported even more conclusive results. Of 120 addicts admitted into the program, only 13 failed to respond adequately to the methadone treatment—a success rate of 89 per cent.[3] Dole and Nyswander reported that "a majority of the patients continuing on the program (71 per cent) are employed in a steady job, going to school, or both."[4] Furthermore, they claimed that "heroin use has been stopped consistently," and "the blockade treatment . . . has virtually eliminated criminal activity" among the patients.[5] Since "before entering the program all patients had been involved in illegal activities," Dole and

* All footnotes may be found at the end of this article.

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Nyswander estimated that the reduction of crime and addiction which resulted from their program had "already saved the community three million dollars" (or $27,470 per patient treated).[6]

To explain how methadone worked to "virtually" eliminate criminal behavior and heroin addiction, Dole and Nyswander further developed the theory of the "narcotic blockade," which they identified as a "new procedure, not to be confused with giving of narcotic drugs to addicts for self-administration." Rather than describing methadone as a synthetic or narcotic (as it was officially designated), they termed it "an antinarcotic agent" which "blocks the euphorogenic action of heroin and other opiates." Thus, after being "stabilized" on methadone, "blockaded patients lose interest in narcotic drugs and are able to work and live in the city"—presumably without seeking heroin or committing thefts to pay for it.[7]

In a follow-up study in 1968 entitled "Successful Treatment of 750 Criminal Addicts," Dole and Nyswander reported that "a four year trial of methadone blockade has shown 94 per cent success in ending the criminal activity of former heroin addicts."[8] And in 1969, the Methadone Evaluation Unit, which had been set up by the sponsors of the Dole-Nyswander program to provide an independent evaluation of its results, concluded that the methadone program had substantially reduced the "anti-social behavior" of the addicts treated, and that the majority of these remaining in the program had become "productive members of society as measured by schooling and employment records."[9]

The authoritative claim that a new drug—an "antinarcotic agent"—had the power to eliminate heroin addiction and criminal behavior resounded in the press, and stirred the popular imagination. An article in *Look* magazine, for example, depicted methadone as a "Cinderella drug" that induces "miracle change" in heroin addicts, explaining that "a teaspoon of [methadone] medicine taken daily in a cup of orange juice is changing former dope addicts into decent law-abiding men."

Although some controversy persisted about the ethics of substituting one form of addiction for another, the putative advantages of methadone to society at large, reported under such headlines as "Methadone Found To Reduce Crime," were generally accepted as established fact. Amidst the continuing reports of methadone's success, and an increasing public concern about urban crime and heroin addiction, the idea of an easily dispensable chemical solution to urban problems took on new force and plausibility. Within one year after the Methadone Evaluation Unit reported its finding, 64 different offi-

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cial and quasi-official groups had moved to set up methadone programs in their respective communities.

Reevaluating the Dole-Nyswander program

But though methadone seemed to readers of the popular press to be the "magic bullet" that ended addiction, in reality it was only part of a program of recruiting, rehabilitating, and monitoring addicts. The successful results of the Dole-Nyswander project could not be entirely attributed to the chemical properties of the drug. Aside from the dispensing of dosages of methadone dissolved in fruit juices, the program included such other elements as the admission criteria for addicts; the medical, psychiatric, and counseling services rendered to patients; the incentives provided for remaining in the program; the rules under which patients were discharged for misconduct; and the standards and methods by which the program was statistically evaluated. To fully account for the early achievement of the Methadone Maintenance Treatment Program, as the Dole-Nyswander project was officially called, it is necessary to examine the other variables in the program more closely.

Unlike detoxification programs, which admitted almost all the addicts who applied for withdrawal treatment (and some non-voluntary cases remanded by the courts), the Dole-Nyswander experiment carefully screened the volunteers for methadone maintenance, and admitted only the relatively small proportion of addicts who met their criteria for treatment. For example, out of 1,233 applicants from the Morris J. Bernstein Institute—the major source of addict-patients—only 521 were admitted for methadone maintenance treatment. Those admitted tended to be older (more than half of the group from the Bernstein Institute were over 32), "whiter" (there were over twice as many whites as blacks in the group), and less recently involved in criminal activities.[10] Moreover, all accepted applicants showed a strong motivation towards accepting methadone treatment, as demonstrated by the fact that they volunteered and remained on a waiting list for six months to a year.

The fact that the methadone recipients were considerably older on the average than street addicts at large was itself an important predeterminant of the group's subsequent performance. More than two thirds of the addicts selected by Dole and Nyswander were over 30 (and 17 per cent were over 40), whereas only one third of the addicts listed in New York City's "Narcotic Register" were over 30.[11] Since criminal behavior (as well as other forms of risk-taking) gen-

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erally tends to decline after the age of 30, and since a substantial proportion of addicts begin to "mature out" of heroin use of their own accord when they reach their mid-thirties, one would expect to find declining rates of criminal offenses (which include drug charges) and heroin use among a group of addicts in their thirties and forties, even if they were not given methadone.

The statistical measures of patient success in the Dole-Nyswander project were further inflated by the constant expansion of the treatment program, which grew from 22 patients in 1964 to 1,800 in 1969, to 4,376 in 1970. The influx of new patients, who comprised a large proportion of the total at any given time, gave the program a much higher "retention rate" than it might otherwise have had. In 1969, for example, Dole and Nyswander claimed that 82 per cent of all patients accepted in the program since its inception had remained on methadone treatment—and they defined this "retention rate" as a key index of success.[12] Because of the enormous expansion of the program in 1969, however, most of the patients in the "retained" category had actually been admitted to treatment only six months before. When groups of patients who had been in the program for more than one year are analyzed, a much lower "retention rate" is obtained (though it is still much higher than the retention rates that had previously been achieved in detoxification programs). For example, of the cohort admitted 48 months before the evaluation, only 58 per cent remained in the program; and of the cohort admitted 33 months before the evaluation, only 66 per cent were retained. In other words, the magnitude of the actual rate of attrition was partially masked by the large numbers of newly admitted patients.[13]

Moreover, most of the departing patients were discharged because they were arrested or otherwise failed to conform to the strict rules of the program, which required that patients seek employment, abstain from illicit use of drugs, and not engage in criminal or anti-social behavior. The dropping of a substantial number of unemployed patients, drug-takers, and law-breakers obviously improved the overall statistical performance of the remaining group. Consider, for example, the claim that "the majority of patients have become productive citizens."[14] Some 26 per cent of the men were employed before admission to the methadone treatment program, and, according to the data compiled by Dole and Nyswander, 66 per cent were employed after one year's treatment, and 78 per cent after four years in the program. This employment rate, however, applies only to the patients who remained in the program. If the discharged patients and drop-outs are included, a much lower employment rate is obtained. Of the *total* number of

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patients who entered the program four years before the evaluation, only 45 per cent were employed (contrasted to 78 per cent of those *retained* in the program).[15]

The methadone program also was not as effective at eliminating drug abuse among the patients as the claims of Dole and Nyswander implied. Indeed, the claim that "heroin use has been stopped consistently" rested on a quite subjective interpretation of what constitutes the usage of heroin. According to studies conducted by Dole and Nyswander themselves, urinalysis samplings revealed that 45 per cent of their patients took heroin (at least once) while receiving methadone, and 15 per cent continued to use heroin repeatedly.[16] (Presumably, this rate of heroin abuse would have been even higher if a substantial number of those who persisted in drug abuse had not been discharged from the program—and thus from the statistical picture as well.) Dole and Nyswander accounted for this apparent contradiction by contending that since methadone works as an "antinarcotic" by "blockading" the euphoria induced by heroin, those who continued to take heroin were merely "testing" the blockade rather than "using heroin" per se. If, however, "using heroin" is defined simply as ingesting heroin—which can be objectively tested by urinalysis —it cannot be maintained that methadone treatment "stopped" heroin abuse.

The reduction in crime

The most impressive and widely publicized statistics presented by Dole and Nyswander suggested that the criminal behavior of addicts had been "virtually eliminated" through methadone maintenance treatment.[17] They claimed a 98 per cent decrease in incarcerations, a 90 per cent decrease in convictions, and a 94 per cent success rate in ending the criminal activities of patients.[18] Since patients who persisted in criminal activities tended to be dropped very early from the program, and thus were not counted in the statistical appraisal of those in treatment, some decrease in the overall crime rate of the patients was to be expected. But a reduction of more than 90 per cent in incarcerations, convictions, and criminal behavior cannot wholly, or even in large part, be accounted for by attrition from the program.

This does not mean, however, that the treatment itself produced this result, for the measures that give rise to these impressive statistics are questionable. Incarceration, for example, measures not only criminal behavior but also judicial behavior, the sentences that judges mete out for crimes. And in the case of violations of narcotic laws, the

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most common offense among addicts, a study by the Center for Criminal Justice at the Harvard Law School shows that New York City judges tend to give much less severe sentences to addicts enrolled in a methadone program than to addicts at large.[19] In fact, the study found that an addict at large is four times as likely to be incarcerated for a violation of the same magnitude as an addict in treatment, and that only one out of 20 addicts in treatment was actually sentenced to prison. Since addicts in treatment are less likely to be incarcerated because of the very fact that they are in a treatment program, a lower incarceration rate does not necessarily mean a lower crime rate.

Moreover, the "90 per cent drop in rate of convictions" also drastically overstates the effectiveness of methadone in reducing the number of convictions of those admitted to the program. The "rate of convictions" is based on the total "man-years" of treatment, a figure obtained by multiplying the number of patients under treatment by the time each spent in the program. An addict who remained in the program for four years, for example, would count as 48 man-months or four man-years, while an addict discharged from the program after one month would count for only one man-month. Obviously, such a formula gives much heavier weight to long-term patients than to those discharged from the program (and anyone convicted was automatically discharged); the rate of convictions, then, tends to diminish accordingly when it is presented as a per cent of the total "man-years."[20] As for the claim that the methadone program resulted in 94 per cent of the addicts being arrest-free their first year of treatment, it is undercut by the fact that 80 per cent were *not* arrested during the year *prior* to their admission. The improvement in the arrest-free record was thus only 14 per cent. On the other hand, the fact that 20 per cent of the addicts were arrested the year prior to admission, and only six per cent were arrested after they were in the program three months or longer is certainly significant, and does seem to indicate a real reduction in criminal activity.[21]

Even here, however, the significance of this reduction in crime is somewhat unclear because the overall statistics are not broken down by criminal charges. Because no distinction is made between arrests for narcotics violations (such as possession of heroin) and arrests for crimes against persons and property, it is not possible to ascertain to what extent the decline in arrests of methadone users reflects merely a reduction in illegal heroin purchases (which would be expected with the free distribution of methadone), and to what extent it reflects a reduction of street crime such as theft, robbery, and assault. This is particularly important since methadone is justified in

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terms of its power to reduce street crime. Dole and Nyswander, for example, opened the Fifth National Conference on Methadone Treatment in Washington in 1973 by asserting: "As a conservative estimate, methadone programs in the New York area . . . are saving the community $1,000,000 per day in prevented crime."

A different assessment—the ARTC program

A quite different picture of the impact of methadone treatment on crime emerges from a study by the Center for Criminal Justice of 416 addicts enrolled in the Addiction Research and Treatment Corporation (ARTC). These addicts, like those in the Dole-Nyswander project, were drawn from the poorer sections of New York City, and were provided with medical, counseling, and psychiatric services as well as a daily dosage of methadone; and also like their counterparts in the Dole-Nyswander project, they showed a marked decline in criminal behavior—a 20 per cent decrease in the *overall* crime index of addicts after one year in the methadone program, measured by their record of police arraignments.

But these ARTC gross crime statistics, unlike the Dole-Nyswander totals, were "decomposed," so that they present a fairly detailed analysis of which sorts of crimes were reduced—and for which age groups: Among the addicts younger than 31, the only reductions in criminal arraignments one year after they had begun methadone treatment (compared with their record one year prior to treatment) occurred in three categories: drug offenses—which was by far the largest component in the total reduction—forgery, and prostitution. In all other categories, the rate of criminal charges filed against younger addicts actually *increased*, even though they were receiving daily dosages of free methadone. Robbery charges quadrupled; assault charges were up by almost 50 per cent; and even burglary and property theft charges increased after one year of methadone treatment. In fact, the only decline in street crime charges occurred in the group over 31, who were mostly in their late thirties and forties (and who would be expected to decrease their crime rate with aging, even if they were not receiving methadone.)[22] Moreover, since many of the patients were remanded to the program by the courts, the year before their entry into the program tended to be the peak year for accumulating criminal charges. If the full period of a patient's addiction is taken as a measure, rather than merely the peak year, the level of criminal charges actually was higher after one year of methadone than it was during an *average* year on heroin.[23]

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One possible explanation for the pronounced increase in street crimes committed by younger methadone users is that the substitution of methadone for heroin made the criminally prone members of this group less sedentary, and thus more active and efficient in pursuing their careers in crime. Professor Irving F. Lukoff, the director of the evaluation team monitoring the project, suggests that the crime pattern is better explained in terms of chronological aging than by methadone use: "With or without methadone or heroin, men seem to increase their criminal activity in their twenties, and decrease it in their thirties." In any case, Lukoff came to quite different conclusions than Dole and Nyswander: First, "Arrest rates alone tend to exaggerate the decline of criminal behavior;" second, "The decline in criminal behavior—in fact any conformist behavior—is contingent on increasing chronological age" (and not necessarily methadone use); and, finally, "A large part of the decline in crime is a function of the unusually high rates in the period preceding entrance, and possibly the reason why many enter treatment. The largest part of the decline is attributable to fewer arrests for drug charges." [24]

Explaining the disparity

Why should two methadone programs in New York City, both dealing with criminal addicts, yield such different results? It is true that the addicts in the Dole-Nyswander program were maintained on a much higher daily dosage of methadone than the addicts in the ARTC's program; but a series of "double-blind" experiments, in which neither doctors nor patients know who is receiving high or low dosages, has shown that the higher dosage has almost no effect on criminal behavior and arrests.[25] The differing results would thus seem to stem from different modes of administration and evaluation. Dole and Nyswander employed far stricter criteria for admittance and continuance in the program than the ARTC, and such selection procedures, by excluding or discharging the addicts most likely to continue criminal careers, may have accounted in part for their higher success rate. It is also possible that the Dole-Nyswander project had superior counseling and rehabilitative services (though it is difficult to compare the quality of such services).

To a large extent, however, the differences between the two programs' crime rates seem to be a statistical artifact. Rather than relying entirely on police and official records of criminal behavior, as the ARTC did, Dole and Nyswander depended heavily on their own counsellors in the program (who were mainly ex-addicts themselves)

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to compile the record of criminal activity by the patients. Lukoff suggests, on the basis of other studies of the problem, that this probably led to a substantial underreporting of criminal charges. And Dole and Nyswander then used measures such as "man years" and "convictions" which tended further to exaggerate the decline in crime.

In any case, the reduction of crime—if any—and other anti-social behavior does not result simply from distributing the drug methadone to addicts. As Dr. Jerome H. Jaffe insightfully pointed out at the time he was directing the Illinois Drug Abuse Program: "It is perhaps unreasonable to infer that methadone by itself produces a change in life style. Changes in criminal activity and gainful employment are more fairly viewed as measures of the efficacy of the rehabilitative system rather than of oral maintenance procedures per se."[26]

The metabolic theory of addiction

Methadone maintenance has been advanced not only as a means of reducing urban crime and unemployment but also as a medical treatment for heroin addicts. The theory which underlies—and medically justifies—this form of treatment holds that heroin abuse, under certain conditions, induces a permanent change in the nervous system that can be compensated for only by the ingestion of heroin or other opiates. Without opiates, addicts with this metabolic disorder are physiologically unable to function, and are medically abnormal. Since the metabolic change produced by heroin abuse cannot be reversed, the only possible medical treatment is to maintain the patient on a less harmful opiate than heroin for the duration of his life. Dr. Dole, who fully developed the metabolic theory of addiction, explains:

> Methadone, when properly prescribed, acts as a normalizer rather than as a narcotic. In this respect the treatment is similar to other maintenance therapies used in medical practice for treatment with patients with chronic metabolic disorders. The medical analogies are numerous—insulin for the diabetic, digitalis for the cardiac patient, cortisone for the arthritic, diphenylhydantoin for the epileptic, etc. Patients with these chronic diseases are dependent on their medication for their normal functioning. The methadone patient, who is also dependent on the daily dose of his medication for his normal functioning, is in the same medical status."[27]

But unlike diabetes, heart disorders, arthritis, and epilepsy, which are associated with readily discernable physiological symptoms, the putative metabolic disorder caused by heroin does not have physio-

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logical manifestations detectable by any medical tests. If addicts are receiving a sufficient daily amount of opiates, the metabolic change presumably is fully compensated for; and, if addicts suffering this disorder are withdrawn from opiates completely, the only symptom is a desire or "craving" for opiates (that cannot be measured by medical instruments).

An immediate problem with the metabolic theory of addiction is that it does not explain a large proportion of the cases. Consider, for example, the Army's experience with addiction in Vietnam. An estimated 35 per cent of the enlisted personnel in Vietnam used heroin at least once, and 20 per cent—or about 100,000 soldiers—considered themselves to be "addicted" at one time. Even after urine testing was instituted in 1971, some 12 per cent of soldiers scheduled to return to the United States failed the test, and were detained in Vietnam. As Dr. Jerome Jaffe noted, "This group was not only experimenting with heroin, but they were apparently using it so heavily that they couldn't stop even when they knew they would be delayed in going home."[28] Yet, after they withdrew or were "detoxified," more than 90 per cent did not relapse into using heroin, and did not experience a continued "craving" for the drug or any other disability.[29]

Follow-up studies showed that "fewer than one in 10 of these soldiers had developed an addiction problem at home eight to 12 months after their return."[30] Dr. Jaffe explained that "we are learning that you can be addicted in one environment, and that if your environment changes enough, or your peers change enough—and if the addiction is not the kind that comes with hardline, intravenous injection—it may be possible to stop using [heroin]."[31] In other words, the Army found that in at least 93 per cent of the cases, heroin addiction was the result not of a permanent metabolic disorder, but of environmental factors such as the availability of heroin or peer group influences.

Similarly, hard-core addicts serving prison terms have been found to function after "detoxification" without any of the disabilities or drug-craving behavior predicted by the metabolic theory. (Once they are released from prison, and heroin is again available, the ex-addicts may become readdicted, but this is perfectly consistent with the environmental theory of addiction.) And since it is estimated (though the data on this question is very limited) that a large segment of all heroin addicts "mature out" of addiction on their own after the age of 35,[32] the theory of a permanent metabolic change could, at best, apply to only a small fraction of even the hard-core addicts. Furthermore, the Dole theory provides no diagnostic method

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of determining whether a given case of addiction stems from a metabolic disorder or from environmental or psychological reasons. Doctors dispensing methadone therefore would have no way of knowing whether they are medically treating a metabolic disorder or simply prolonging an addiction. The fact that doctors, including Dole and Nyswander, generally feel justified in denying methadone to patients who fail to conform to the rules of the program (by discharging them) suggests that not even proponents of the metabolic theory take the analogy with diabetes seriously. (Certainly, a doctor would not feel justified in denying insulin to a diabetic.) The theoretical underpinnings of the view that methadone is a "normalizing" medicine thus remain highly questionable.

The "blockade" theory

Even if the metabolic theory has only limited application, however, methadone could still be considered a medicine if it prevented or "blockaded" an addict from using other more harmful drugs (in the same sense that gamma globulin acts as a preventive medicine in protecting someone against hepatitis). The "blockade" theory, it will be recalled, holds that a high dosage of methadone acts as an "antinarcotic" by preventing addicts from receiving any euphoria or other positive reinforcement from heroin. This claim of a "blockade" effect is problematic at best: Methadone neither prevents the heroin from reaching the receptors in the brain (which is usually what a "blockade" infers), nor cuts the addict off from the sedative and pain-killing effects of heroin. Instead, according to the Dole-Nyswander theory, high dosages of methadone build up the addict's "tolerance" for opiates to the point where he will no longer receive a euphoric feeling from taking a standard-size dose of heroin. This form of "blockade" is analogous to feeding a "strawberry ice cream addict" a sufficient number of gallons of vanilla ice cream to prevent him from receiving pleasure from additional quantities of strawberry (assuming that there is as much "cross-tolerance" between ice creams as there is between opiates.) Since the euphoria-"blockade" is difficult to measure objectively—patients are either asked about the euphoria or observed by ex-addicts employed by the program—and since addicts, especially older ones, commonly take heroin for its sedative and pain-killing effects, there is simply no way of proving or disproving the euphoria-"blockade" theory.

Doctors Dole and Nyswander predicated this "blockade" theory on the early results achieved in their program and on the reports of

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the ex-addicts serving as counsellors to the methadone patients. Other researchers subsequently found, however, through daily urinalyses of methadone maintenance patients that they continued, and even increased, their usage of narcotics while receiving high dosages of methadone. In extensive month-long examinations of a sample of patients drawn from a maintenance program in Philadelphia, Carl D. Chambers and W. J. Russell Taylor found (through urinalyses) a remarkably high pattern of "cheating." Some 77 per cent (all of whom had been in the program six months or longer) were still using heroin, 30 per cent were using barbiturates, and 25 per cent amphetamines. (During the same period, the counsellors on the program reported that only 40 per cent were using heroin, 7.5 per cent barbiturates, and five per cent amphetamines—indicating that they heavily underreported drug usage).[33]

Nine months later the same sample was again tested for a month-long period. This time 92.3 per cent of the methadone patients were found to be using heroin, 43.6 per cent barbiturates, 69.2 per cent amphetamines, and 43.6 per cent cocaine.[34] Moreover, nearly two thirds of the patients had "dirty urine" (i.e., evidence of drug abuse) half the time they were tested. Chambers and Taylor concluded:

> . . . [E]ven after a year on relatively high dosages of methadone, neither a narcotic blockade had occurred nor had drug craving significantly diminished. These findings must also be viewed within the context that neither the patients themselves nor the program within which they were being treated are significantly different from most other methadone programs or patients.[35]

And similar patterns of "cheating" have become apparent with the application of more sophisticated urinalysis techniques to a wide variety of other methadone programs.

The new justification: methadone as chemical parole

By the time that the Fourth National Conference on Methadone Treatment was convened in 1971, there was little support for either the "metabolic disease" or the "blockade" theories. Indeed, almost all the empirical evidence presented at the conference tended to undercut them, and even the strongest proponents of methadone expressed doubts about the original medical justifications for its use. Dr. Avram Goldstein, the highly respected director of the Addiction Research Laboratory at Stanford, demonstrated through data drawn from his California methadone programs that "the dose of methadone is largely irrelevant." Finding no empirical basis for the "metabolic

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disorder" theory, and rejecting the "blockade" theory as "confusing," he concluded:

> Methadone cannot magically prevent heroin use in a patient who wants to use heroin; it can only facilitate a behavior change in people who have made a conscious decision to change. Thus, the paramount feature of a successful methadone program is what it does in ways *other than chemical* to help the patient rehabilitate himself (italics in original).

But if methadone neither compensated for a metabolic deficiency nor prevented continued heroin abuse, what was the advantage of maintaining addicts on methadone? Dr. Goldstein reasoned that "the sustained methadone dependence" served to break the "conditioned behavior" surounding heroin use, and to bring "the patient into the clinic daily, ensuring regular contact with the clinic staff, and introducing a degree of regularity into his disordered life." Thus no attempt was made to justify methadone as a "normalizing" or a preventive medicine; instead, it was defined principally as a powerful means of exerting a form of social control over street addicts. Not only did it attract addicts into participating in government-sponsored programs, and transfer the object of their dependency from an opiate procured illegally to an opiate obtained from licit sources, but it placed them on a highly effective kind of chemical parole. Their urine could be tested daily for illicit narcotics, and if they broke their parole—or the rules of the program—they could be denied the methadone they had become dependent on.

Methadone thus had changed in concept from a form of medical treatment, analogous with the insulin treatment of diabetes, to an incentive for rehabilitation programs. Signalling the change, Dr. Goldstein ended his presentation by cautioning:

> Nothing I have said should be interpreted as "debunking" methadone or derogating its importance. It is a fantastically effective tool for bringing addicts into a new and helpful kind of therapeutic environment. If they have sufficient motivation to enroll in a methadone program, that motivation can be nurtured by a good staff, can be transformed into a real desire for an alternative life style, more satisfying than that of a "junkie." No matter that the "metabolic disease" concept may be incorrect.

Methadone as public policy

In January 1969, when Richard Nixon assumed office, there were only 16 drug treatment programs receiving financial support from the federal government, and the main forms of treatment were detoxification and drug-free therapy. Almost all the agencies involved in the

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administration of these treatment programs, including the National Institute of Mental Health, the Office of Economic Opportunity, and the Department of Housing and Urban Development, were actively opposed to methadone maintenance. By 1973, however, the federal government was funding some 394 treatment programs offering methadone maintenance. In the intervening four years, there had obviously been a major shift in policy towards methadone.

To some extent, the expansion of treatment programs was a response to an expansion in the addict population during the 1960's, but the shift from detoxification to drug maintenance reflected a redefinition of the problem of addiction itself. Up until the mid-1960's, heroin addiction was generally thought to be a problem chiefly for the individual addict, and treatment therefore was aimed at freeing him from his dependence on the drug. By the early 1970's, however, heroin addiction had increasingly come to be regarded less as a problem for the individual addict than for society at large, since it was presumed that addicts were compelled to commit crimes to pay for their supply of heroin. The focus of policy thus changed accordingly from relieving the individual from the suffering and degradation of drug dependency to relieving the rest of society from the putative criminal behavior of addicts. As James Vorenberg and Irving Lukoff correctly noted, "the core of community concern is the relationship between addiction and crime."

Given this new view of the problem, the solution involved bringing the addict population under some form of social control. Since methadone maintenance, whatever its side effects, promised to transfer street addicts from a dependency on heroin, which they had to obtain at great cost from illegal suppliers, to a dependency on methadone, which could be dispensed legally under tight controls, it held great appeal to the young men in the Nixon Administration concerned with the problem of urban crime. Egil Krogh, then a deputy assistant to the President for domestic affairs, with special responsibilities for law enforcement, explained the Administration's interest in methadone in 1971 in the following terms:

> . . . [W]e found there was a cause and effect relationship fairly clear between heroin addiction and the need to commit crimes to support the habit. So we felt we needed to greatly expand the capability of the District of Columbia to treat those with the problem. . . . After a year we found that those [addicts] in high-dosage methadone had a marked decline in criminal recidivism. . . . I cannot piece out exactly what is attributable to narcotics treatment or police work, lights, a new court, but we feel all taken together have led to that result [decreased crime] and we would like to expand that type of treatment across the country.[36]

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A Special Action Office for Drug Abuse Prevention was thereupon created, under Dr. Jerome Jaffe, to coordinate the expansion of "multimodality" treatment programs (which allow addicts to take advantage of either methadone maintenance or drug-free treatment).

Has methadone worked?

By 1973, the government had succeeded in expanding treatment programs to the point where virtually any qualified heroin user could receive methadone maintenance, if he wanted it. The waiting-lists for methadone had all but disappeared, and some 73,000 people were enrolled in maintenance programs across the United States. Yet as results were evaluated and compared with police data, it became clear that the widespread distribution of methadone was not going to solve the problems of crime, unemployment, or even drug addiction. To be sure, a large number of addicts were brought under some form of social control and, where urinalyses were effectively monitored, placed on a kind of chemical parole. But the main reduction in crime brought about by the use of licit methadone was in the realm of narcotics offenses (such as drug possession charges), which helped improve the appearance of overall police statistics, but did little to allay public concern over robbery, mugging, assault, and other street crime. Indeed, in 1973, despite the claim that the addict population had been freed from the compulsion to buy illicit heroin by the availability of methadone, there was a significant increase in the FBI crime index.

In addition, while heroin consumption may have decreased as methadone became more freely available—and certainly deaths attributed to heroin decreased—there is little evidence to suggest that overall drug abuse was reduced. The pattern found in most methadone programs was one of "poly-drug" abuse: When addicts were denied the euphoric reinforcement of heroin because methadone raised their tolerance to opiates, they quite commonly turned to amphetamines, cocaine, barbiturates, or other non-opiates. Moreover, the "leakage" of methadone from the maintenance programs into the illegal market created an entirely new drug abuse problem. In 1974, the Drug Enforcement Administration reported that deaths from illicit methadone have surpassed deaths from illicit heroin, and that methadone now constitutes a substantial share of the illegal traffic in drugs. At best, 7.5 million doses of methadone distributed annually by government-licensed clinics seem to have had the effect of shifting drug abuse from heroin to other equally damaging drugs.

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Nor did methadone bring about the wholesale conversion of unemployed heroin addicts to productive members of society. While results varied widely from program to program, few of the newer programs even approached the high employment and retention rates claimed by Dole and Nyswander. For those strongly motivated to change their style of life, methadone no doubt assisted in their rehabilitation by lessening the compulsion towards heroin use. But for the majority of methadone users, who continued to use other drugs and to drop in and out of programs, methadone was merely one more drug, freely obtained, which could be used to maintain their "habit." Methadone failed to achieve the predicted results, not merely because the programs were inefficient or expanded too rapidly, but because the basic assumption that it could somehow preclude drug abuse, and force addicts to convert, proved to be untrue.

The unintended consequences

As policy, therefore, methadone maintenance rests on shaky foundations. Originally, or at least when maintenance was first proposed in the 1960's, it was widely assumed that a large proportion of heroin addicts suffered from a permanent metabolic disorder which required that they be maintained on drugs for the duration of their lives. Subsequently, however, this theory was all but abandoned by most researchers in the field (or so diluted by qualifications that it no longer justified drug maintenance). And if it is true that a large proportion of heroin addicts would "mature out" of heroin use after the age of 35 without the assistance of methadone, then the policy of admitting almost all heroin users who desire methadone to maintenance programs may have the unintended consequence of prolonging addiction and increasing the population of addicts as a whole. Nor could this problem be solved by more stringent entrance criteria, since, as Dr. Jaffe points out, there is no way to determine in advance if a candidate for a methadone program would be likely to abandon heroin use on his own. Moreover, it is now widely believed, after the Vietnam experience with addiction, that availability is a powerful, if not the main, determinant of drug use. If drugs are not available, a large proportion of the "users" will abstain from seeking them out. The policy of making methadone readily available, then, may encourage, rather than discourage, drug use among a large group of persons who otherwise would abstain.

The collapse of the "blockade" theory in the face of empirical findings also presents a dilemma for methadone policy. If high dosages

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of methadone merely raise the tolerance for opiates of drug users, and drive them to other drugs for the euphoria methadone temporarily denies them, then the problem of heroin addiction is simply being expanded into a problem of poly-drug abuse, and it becomes difficult to justify the methadone policy as a "drug prevention" program. To be sure, methadone users can be placed under a tighter form of chemical parole through more sophisticated urinalysis techniques and more rigorous requisites for remaining in the program; however, such forms of behavior control raise the most difficult sort of ethical and legal questions: Can the state be justified in deliberately raising the tolerance of an addict for drugs by giving him higher dosages of an opiate than he would normally take, and then denying him the opiate when he fails to conform to the rules? At the very least, this would seem to be a form of punishment not proceeding from any "due process of law."

The alternative is to provide the addict only with a lower dose of opiates than he would otherwise take, thereby not increasing his tolerance. But low-dosage methadone programs do not seem to provide sufficient incentives to hold most hard-core addicts for any period of time. Most evaluations have found that low-dosage programs have had mainly transient populations who enter and leave the program within six months. Applying tighter controls against heroin and other forms of drug abuse in low-dosage programs would thus probably have the consequence of accelerating the turnover, and driving the hard-core addict away from the rehabilitation aspects of the program. In fact, as Dr. Avram Goldstein has pointed out, the only sort of addict who would remain in a program with low dosages and strong controls would be one who was strongly motivated to change his life style; and in this case, gradual withdrawal through reducing the dosage of methadone might be a more logical form of treatment than "maintenance."

Further, the massive methadone program—at the local as well as the federal level—tends to focus attention exclusively on heroin, which is only one aspect of the overall drug abuse problem. Methadone maintenance does not discourage the use of non-opiates like barbiturates, amphetamines, and cocaine, and may actually encourage poly-drug abuse. Therefore, agencies and personnel involved in the various methadone programs have a strong interest in emphasizing the menace of heroin—even when the rate of new addiction declines, as it has done in the past three years—and obscuring the dangers posed by other forms of drug abuse. The distribution of methadone by the government thus not only has failed to alleviate the

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problems of urban crime and drug addiction but may even have further complicated these problems by adding another illegal drug to the market (which, to some degree, compensated for the curtailed supplies of heroin from abroad), encouraging poly-drug abuse (by addicts who could no longer derive a euphoric experience from opiates), and prolonging addiction among some addicts who might otherwise have "matured out."

Social problems and chemical solutions

Perhaps the most difficult question in terms of public policy is why the government persisted in the methadone program on the basis of such questionable assumptions and equivocal results. In fact, the medical authorities and consultants who were most heavily involved in advising the White House on the methadone program were intelligent and thoughtful men who understood the quixotic nature of the theories about "blockades" and "metabolic disorders," and who realized fairly early that the mass distribution of methadone would not significantly reduce urban crime or eliminate drug abuse. They believed, however, that methadone would be a powerful lure for attracting and maintaining street addicts in rehabilitation programs. This was their motivation. The White House, however, had other things on its mind. It was interested mainly in crime. The experts therefore "sold" methadone maintenance to Egil Krogh, John Ehrlichman, and President Nixon as an "anti-crime" program, rather than as a "rehabilitation" program. The net result was that those with the technical competence to see the limits of methadone treatment chose not to deflate the unrealistic claim that methadone would substantially reduce crime.

The public, and especially the enlightened liberal community, was beguiled by the unchallenged rhetoric of the program. The notion that most crime was the product not of "criminals," but of "sick" individuals who could be cured by the distribution of an inexpensive medicine was most appealing. It fostered the hope among a large segment of the middle class that street crime could be eradicated by medical means. Unfortunately, criminal behavior—and addiction—proved to be more complex problems that did not lend themselves to simple chemical solutions.

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FOOTNOTES

[1] Vincent P. Dole and Marie E. Nyswander, "A Medical Treatment for Diacetylmorphine (Heroin) Addiction," *Journal of the American Medical Association*, Vol. 193, No. 8 (August 23, 1965), p. 80.
[2] *Ibid.*, p. 84.
[3] Vincent P. Dole and Marie E. Nyswander, "Rehabilitation of Heroin Addicts after Blockade with Methadone," *New York State Journal of Medicine*, Vol. 66 (1966), p. 2011-2017.
[4] *Ibid.*, p. 2016.
[5] *Ibid.*
[6] *Ibid.*, p. 2017.
[7] *Ibid.*, p. 2012.
[8] Vincent P. Dole and Marie E. Nyswander, "Successful Treatment of 750 Criminal Drug Addicts," *Journal of the American Medical Association*, Vol. 206, No. 12 (December 16, 1968), p. 2708.
[9] Frances Rowe Gearing, "Evaluation of Methadone Maintenance Treatment for Heroin Addiction: A Progress Report," Paper presented before the Epidemiology Section of the American Public Health Association (Philadelphia: November 13, 1969), p. 10.
[10] Marvin E. Perkins and Harriet I. Bloch, "Survey of Methadone Maintenance Treatment Programs," *American Journal of Psychiatry*, 126 (April 10, 1970), pp. 33-39.
[11] Gearing, *op. cit.*, figure 2.
[12] *Ibid.*
[13] James V. DeLong, "Treatment and Rehabilitation," in *Dealing with Drug Abuse*, foreword by McGeorge Bundy (New York, 1973), p. 176.
[14] Dole and Nyswander (1968), p. 2711.
[15] DeLong, *op. cit.*, p. 207.
[16] Dole and Nyswander (1968), p. 2711.
[17] Dole and Nyswander (1966), p. 2017.
[18] Herman Joseph and Vincent P. Dole, "Methadone Patients in Probation and Parole," *Federal Probation*, Vol. 34, No. 2 (June 1970), pp. 42-48; Dole and Nyswander (1968), pp. 2710, 2708.
[19] Gila J. Hayim, "Changes in the Criminal Behavior of Heroin Addicts: A One Year Follow-Up Study of Methadone Treatment" (Mimeographed, 1972), p. 57.
[20] The National Commission on Marijuana noted in reference to these claims: "Some studies use a man-year figure as a measurement of success although the conclusions drawn therefrom are especially likely to be distorted and misleading." See: *Drug Use in America: Problem in Perspective*, (Washington: U.S. Government Printing Office, 1973), p. 179.
[21] Gearing, *op. cit.*, figure 2.
[22] Hayim, *op. cit.*, p. 35.
[23] James Vorenberg and Irving F. Lukoff, "Addiction, Crime, and the Criminal Justice System," *Federal Probation* (December, 1973), p. 7.
[24] Irving Lukoff, "Issues in the Evaluation of Heroin Treatment," A Report to the Epidemiology of Drug Abuse Conference (Puerto Rico: February 12-14, 1973).
[25] In one such study, Lukoff found the correlation between low as opposed to high dosages and arrests to be an insignificant .009. Avram Goldstein in his report "Blind Controlled Dosage Comparisons with Methadone in 200 Patients" (3rd National Conference on Methadone Treatment: 1970) found: "Comparisons of 30, 50, 100 mg. daily dosages revealed surprisingly few differences." Jerome H. Jaffe in "Methadone Maintenance: Variation in Outcome Criteria as a Function of Dose" (3rd National Conference on Methadone Treatment: 1970) came to essentially the same conclusion.
[26] Jerome H. Jaffe, "The Maintenance Approach to the Management of Opioid Dependence" (unpublished, 1971).
[27] Joseph and Dole, *op. cit.*, p. 43.
[28] Quoted in T. George Harris, "As Far as Heroin is Concerned the Worst is Over," *Psychology Today* (August 1973), pp. 71, 75, 76.

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[29] Interview in March 1973 with Richard Wilbur, Assistant Secretary of the Army for Public Health.
[30] Quoted in Harris, *op. cit.*, p. 76.
[31] *Ibid.*
[32] Charles Winick, "Maturing Out of Narcotic Addiction," *UN Bulletin of Narcotics*, Vol. XIV, No. 19 (1962).
[33] Carl D. Chambers and W.J. Russell Taylor, "Patterns of 'Cheating' Among Methadone Maintenance Patients," Paper presented at the Eastern Psychiatric Research Association Meeting (New York: November 7, 1970), pp. 328 ff.
[34] *Ibid.*, p. 331.
[35] *Ibid.*, p. 335.
[36] Press conference of John D. Ehrlichman, Egil Krogh, and Jerome H. Jaffe (Washington, D.C.: June 17, 1971). The degree to which the decline in crime in Washington was due to methadone is somewhat questionable since it began in 1969 when only 99 addicts in the entire area were receiving methadone.

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How many games in town? – the pros and cons of legalized gambling

JESS MARCUM & HENRY ROWEN

ONE of the little analyzed recent changes on the American scene is the rapid spread of legal gambling. As of this writing, 35 states have some form of legal gambling: 31 allow pari-mutuel betting at race tracks, eight have state lotteries, three permit off-track betting on horse races, three permit only charity bingo games, and one has casinos. Changes are coming rapidly. New Hampshire established its lottery in 1963, followed by New York in 1967, New Jersey in 1970, and Pennsylvania, Massachusetts, Connecticut, Michigan, and Maryland in 1971-73. New York passed a law in 1970 permitting off-track betting on horse races on local option, and in 1971 the New York City Off-Track Betting Corporation began operations. New Jersey began a daily lottery and Connecticut legalized off-track betting in 1972. In 1973, voters in Maine approved a lottery. In New York, the first stages of legislation to permit casinos have passed, and a referendum on casinos will be held this November in New Jersey. Moves to legalize gambling are under study in several other states, including Kentucky, Rhode Island, and Georgia.

This process has been driven largely by state legislators' desire for revenues. Not only have expenditures by state and local governments increased rapidly in the past two decades—more so than federal

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spending—but there has been resistance to employing the most important source of local revenue, the property tax. The prospect of getting tens or hundreds of millions of dollars per year from a commodity many people want to consume, without generating taxpayer opposition, has become hard to resist.

The revenue motive has been buttressed by the belief that legal gambling will drive out illegal gambling. Estimates of the amount of illegal gambling vary greatly, but gambling handles in the scores of billions of dollars annually and profits in the billions are frequently reported. Moreover, it is often held that illegal enterprises use gambling profits to enter the narcotics and other criminal trades, to buy into legitimate enterprises, and to corrupt law enforcement personnel. This concern led the Knapp Commission, appointed to investigate corruption in New York City's law enforcement system, to recommend the repeal of criminal laws against gambling.

Supporting these pragmatic arguments is a more philosophical one which holds that it is futile, indeed harmful, to try to ban an activity which is widely demanded and is available illicitly—often with the connivance of the police. Respect for the law is diminished. It is unreasonable and arbitrary to force people who want to gamble legally to go to the race track or to Nevada or the Caribbean—or to speculate in stocks, commodities, or stock options. Gambling is a victimless crime. It makes no more sense to try to prohibit gambling than to try to prohibit the consumption of alcohol. To be sure, some people gamble to excess just as some drink or smoke or eat to excess. But people who don't want to gamble don't have to, and those who do will find some way of doing it anyway; therefore they should have an opportunity to do so legally and conveniently.

Given the currency of this point of view, it is a pretty good bet that there will be a widespread adoption of legal gambling during the next decade. The consequences of such a development, however, are far from clear.

The key questions

The central issue is one of objectives. *The objective of maximizing tax revenues from gambling is by no means the same as the objective of eliminating illegal gambling in order to reduce corruption, or that of minimizing governmental interference with the exercise of personal preferences.* A given policy is unlikely to achieve all of these objectives. Moreover, even if these different goals are reconciled in a given policy, there is likely to be a conflict between it and another

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policy objective that most communities are likely to regard as important: keeping the total volume of gambling activity within reasonable bounds and seeing that it is conducted with a fair degree of public decorum.

The resolution of this central issue of objectives depends, as with most complex public policy issues, on a combination of facts, estimates, conjectures, and values. At this stage, although few reliable answers can be given, some important questions need to be asked and the available evidence used. One key question is: How much gambling would take place with widespread legalization? We estimate that the complete legalization of gambling throughout this country would lead to a large increase, probably about three times as much as the total amount of legal and illegal gambling that goes on today.[1]

Another key question is: How important are gambling revenues likely to be to state and local budgets? Here we conclude that the contribution from universal legalization would be modest, amounting at most to about five per cent of total state and local tax revenues.

Some other important questions on which some evidence is available and conjectures can be made are these:

- What would be the social consequences if a large increase in gambling occurs? For instance, how serious a problem might gambling addiction become?
- What would be the distribution of gambling losses across income groups? What would be the incidence of gambling taxes?
- Gambling-associated corruption in some communities (e.g., New York City) seems to be a serious problem. But is law enforcement corruption by gambling interests a serious problem across the nation? Is widespread infiltration of legal gambling enterprises by criminals likely to occur, and how might it be prevented? Are abuses similar to those that led virtually all forms of legalized gambling to be prohibited during the course of the 19th century likely to reemerge?
- How attractive does legal gambling have to be to drive out illegal operations? In particular, how much of a tax burden can the legal version bear and remain competitive?
- What should be the role of government? Should it be a prohibitor, regulator, operator, tax collector? Should public-benefit, non-profit corporations be established as operators? Should gambling be left largely to the market?
- What can be done to preserve public decorum? For example,

[1] A brief description of the methods used for estimating the present level of illegal gambling and the potential level given legalization is available upon request from the authors.

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should gambling be intensely promoted and displayed as it is in Nevada or even as it is by the Off-Track Betting Corporation (OTB) in New York City?

- What should be the role of the federal government as the states increasingly compete with each other in offering gambling opportunities?

Varieties of gambling

In addressing these questions it is important to keep in mind variations in the characteristics of different games and in gambling behavior. There are important differences among lotteries, numbers, off-track betting, sports betting, and casinos, and also in the traditions and behavior of the citizenry of different communities. These need to be examined with some care if the consequences of legalization are to be understood.

Horse and dog racing. Betting on horse racing has been legal in many states for years. (Greyhound racing accounts for only a small fraction of all racing revenues and has much less growth potential than other types of gambling.) This acceptance is in part a class phenomenon; as the "sport of kings" it is distinguished from the more common forms of gambling. Moreover, since tracks are usually not easily accessible, the time and expense of going to them requires premeditation, which tends to discourage impulsive betting.

In 1971, revenues to the states from horse and dog racing were $550 million, and about an equal amount went to the tracks. This billion dollar take from the pari-mutuel pools comprised about 18 per cent of the $5.5 billion wagered.

How much might on-track betting be expanded? If racing were to be legalized in the 19 states which forbid it, night racing permitted universally, and additional racing dates granted (continuing a trend), we estimate that the volume and revenues of on-track betting would about double. Estimates of this kind are, of course, dependent on several uncertain variables. For one thing, the popularity of horse racing is not growing, and among young people it is actually declining. For another, the amount of on-track betting depends on opportunities for off-track betting. Betting on horses and on dogs interacts, and there are also interactions with other types of gambling. But if our estimate is correct, an additional $500 million in government revenues would probably be generated.

Definitions are important in understanding gambling, and such cat-

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egories as "handle," "take," and "revenue" are often confused in the various types of gambling. In horse racing the categories are simple: The total amount wagered in the pari-mutuel pools is the "handle"; the total amount taken out of the pool before winners are paid off is the "take," which is equivalent to the gross profit; and "revenue" refers to the amounts that governments receive.

Finally, significant changes are taking place in horse race betting. Combination bets offering very-high-odds payoffs are becoming increasingly popular. The prototype is the daily double, which pays off on the winner of the first and second races as parts of a single combination wager. Payoffs average around $100 for a $2 ticket, in contrast with the average individual winner paying around $14. In recent years, many new forms of combination betting have appeared—Quinellas, Perfectas, Exactas, Twin Doubles, Five-Ten, Superfectas, and the like. In the Superfecta, Twin Double, and Five-Ten forms of wagering, the payoffs may become astronomical—$100,000 or more for a $2 wager.

These exotic bets are, in effect, lotteries and the reason for their popularity is clear: They offer a chance to win a fortune on a very small wager. The large payoffs also provide an incentive to engage in fraud. But the public's memory is usually short. Wagering and attendance fall off for a month or two after a fraud is exposed and then recover; however, the recent scandals at tracks in New York have led some tracks to abandon lottery-like bets.

Off-track betting. New York's legalization of off-track betting was a milestone in legalized gambling. In fiscal 1973, OTB's handle of $588 million produced gross profits of $99 million, of which $42 million in revenue was distributed to New York City and State. Early projections of government revenues from a mature New York OTB system went as high as $200 million a year, but recent estimates have been much lower. We estimate that off-track betting throughout the country would eventually produce about $1 billion in additional revenues. This estimate amounts to saying that the net revenue from off-track betting would equal that of on-track betting. Our figure for off-track betting could turn out to be too low, but if so, the on-track take is bound to be reduced. (It is not surprising, therefore, that the tracks in New York State have opposed the extension of off-track betting and have lobbied for a larger share of the revenues.) Another way of looking at this is that the total government revenues from full-scale legalization of both on-track and off-track betting would be about $2 billion, with less uncertainty in this total than in the split.

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How does this potential compare with the current level of illegal bookmaking? We estimate net revenues (i.e., after paying off winners and after expenses of operation) to bookmakers at about $500 million, an amount about equal to current government revenues from the tracks. This is far below other estimates which usually assume $5 to $10 bet with bookmakers for every $1 bet at the tracks. (Our estimate, made several years ago, is supported by a recent survey of bettors in New York City made for the Fund for the City of New York.)

The extent to which legalized off-track betting would hurt bookmakers is difficult to estimate. Bookmakers work largely on credit. Some large bettors prefer them because they can wager large sums without changing the odds, and because it is easier to avoid paying income taxes on net winnings. And many bettors prefer bookmakers because they take multiple bets such as parlays and "round robins." On the other hand, OTB has advantages. For one thing, bookmakers have been known to vanish mysteriously. And most give only 15-to-1 maximum odds to win, while OTB has no such limit. Bookmakers are reluctant to take bets on the exotic, high-odds forms of bets. Finally, OTB is, after all, legal. All in all, it is not unreasonable to guess that bookmakers would lose about half of their horse racing business in the event that off-track betting were to become legal and widely available.

Lotteries. The first American lotteries in recent times, in New Hampshire and New York, were not very successful. Then New Jersey showed the rest of the states how to do it right. The New Jersey experience (discounting sales to non-residents) can be extrapolated with fair confidence for the whole country. The result is a potential net revenue estimate for the entire country of approximately $1 billion a year.

Lotteries are coming into fashion for several reasons. Nowadays they are being run by state governments under strict supervision, and, so far, without scandal. They offer the possibility, however small, of becoming rich from only a small wager. The fact that about 50 per cent is taken out of the lottery pool before distribution to winners makes little difference to the players. If one buys a ticket, it doesn't matter much whether the prize is $500,000 or $600,000. If you win, you're rich. Further, the temptation to spend a great deal on lotteries is small.

There is another important consequence of the high odds for lottery players: For the vast majority of players, the sum wagered (or

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the handle) is totally lost. For example, in present pari-mutuel wagering on horses, out of $6 billion bet annually, some $5 billion is returned to the players and all of the players share this $5 billion more or less in proportion to the total amount of their wagers. There is some unevenness due to luck (and much less due to skill), but it averages out more and more as time goes by. In a lottery, no matter how much time goes by, luck never has a chance to average out, even in a lifetime. So although a few get rich by chance, most lose for good. Therefore, from the perspective of the average player the gross take for lotteries is the same as the handle.

There is relatively little illegal wagering on lotteries. It consists mostly of wagers on the Irish Sweepstakes and other foreign lotteries and probably produces illegal profits of no more than about $50 million a year.

Numbers. In the numbers game one may bet an amount from a few pennies to several dollars on any three digit number from 000 to 999. A number is randomly chosen each day (or more often) from some presumably random set of figures issued by the stock market, the treasury, race tracks, or elsewhere. (There are also other bets involving three or fewer digits.) The correct odds for a fair payoff are clearly 999-to-1. But the average numbers bank payoff is about 550-to-1, so that the gross take is 45 per cent. But by the time all expenses have been paid, the net return to the bank is probably about 10 per cent of the handle.

Numbers is an illegal lottery. But numbers has a special appeal because the bettor can pick his own number, the drawings are conducted daily instead of weekly or monthly, very small wagers can be made, and numbers "runners" are generally available wherever players are found.

Our estimate of the present illegal net revenue to the numbers bankers, which is a very approximate one, is about $250 million a year from a total handle of $2.5 billion. This volume is concentrated among blacks and ethnic groups in the older cities, especially in the East and Midwest.

Several states are now actively considering setting up lotteries which would be similar to the numbers game. (New Jersey has a daily lottery, but it does not permit bettors to pick their own numbers). Our estimate for the net revenue to governments that might be yielded by legalized numbers-type lotteries is $500 million. This potential is modest because numbers is not popular nationwide today, and probably would not become so even if offered legally.

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Sports betting. Only Nevada now has legalized betting on sporting events, but proposals to legalize it elsewhere have been advanced. The owners, commissioners, and coaches of sports teams have strongly opposed legal wagering on the grounds that it would offer too many temptations for bribery. More important, it would hurt the image of sports. This opposition will persist, particularly against straight wagering on individual sporting events. (These objections should not be as strong to the lottery kind of sports pool betting popular in Great Britain, or to the football cards popular in the United States.)

In fact, there is a great deal of betting on sports today. Our estimate for the total net revenues from presently illegal sports betting is $500 million a year on a handle of $12 billion. Legalizing all types of sports betting might produce about $1 billion in revenue to states on a handle of about $33 billion. These net figures may seem low, but the gross take is as little as 2.5 per cent on baseball and as little as five per cent on other sports. These percentages are reduced even further when bookmakers are forced to change odds in order to try and equalize the money bet on both sides of a game. The gross percentage taken from all sports events is probably around six per cent of the money wagered. If we assume that half of this gross goes to expenses, three per cent is left. (Although this margin seems small, average sports bets are large relative to horse race bets; therefore the percentage required to cover expenses is much lower.) If sports betting were legalized, this is the share assumed to go to governments.

It is by no means certain, however, that such revenues could, in fact, be generated. Operating sports betting establishments is a hard line of work. Professional oddsmakers must set and move two-way prices, and this is a skilled and risky business. Wagering on sporting events could be run on a pari-mutuel pool basis rather than a fixed-odds basis, but it might be difficult to organize such pools effectively, and the public demand is very uncertain.

Casino gambling. The main casino games consist of dice or craps (which accounts for about one half of casino revenues), blackjack (about one fourth of casino revenues), baccarat, roulette, keno, and slot machines. (Keno is a type of lottery having "numbers"-like characteristics; if casinos become widely accessible, keno might cut into the illegal numbers handle.)

Our estimates of potential net revenues from complete legalization of casino games is $1.8 billion, from a total handle of $36 billion. This

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implies that the United States could ultimately support the equivalent of 10 "Nevadas." (Nevada, in 1971, had a total handle of about $3.6 billion.) In contrast, the present illegal annual net take from casino type gambling is, we estimate, a very much smaller $150 million. Such casino operations have not always been so modest. Illegal casino gambling flourished in the 1930's and 1940's in many states, but these operations have now largely disappeared. The operators in many cases moved to Nevada.

In horse racing, the pari-mutuel take or gross revenue is simply related to the total handle or amount bet. These relationships are more complicated in casino games, and most public discussion on this topic is highly confused. "Handle," more commonly called "drop," is defined in Nevada[2] for such casino games as craps, blackjack, and roulette as the total amount of cash and cash equivalents, such as markers or notes, in the box on each casino table at the end of each eight-hour shift, or more uncommonly at the end of each twenty-four-hour day.[3] The gross profit or "win" of a game is the amount of cash and equivalents in the box minus the deficit in chips from the chip rack. The "percentage hold" is the win divided by the drop. It is calculated for each table, for each type of game, and is summarized for every shift, for every day, and for the year to date. The percentage hold differs for each type of casino game and will, due to chance, fluctuate violently from day to day and even from month to month. At the end of a year, however, the figures settle down to fairly constant values. Craps will ordinarily hold about 18 per cent, blackjack 22 per cent, and roulette about 33 per cent of the drop. These numbers are known only from experience. They vary from casino to casino depending on the rules of the different games, different methods of handling credit play, and the different habits of the clientele.

The percentage hold is dependent on, but *not* equal to, the percentage favoring the house on an individual bet. In roulette, to take the simplest example, all individual bets (excepting one obscure and little used bet) have a common fixed house percentage of 5.26 per cent.[4] The other casino games such as dice and blackjack have vary-

[2] This description applies with only slight differences to Puerto Rico and to casinos in other countries.

[3] Most of the cash in the box has been exchanged for gaming chips from the table chip rack which are used to make bets. Some cash it bet directly, and if the bet is lost, the cash goes into the box. If the bet is won, it is paid in chips. The box is never opened at the table.

[4] If one kept track of each individual roulette wager made and called the total of all such wagers the handle, in the long run the house would hold about 5.26 per cent of the handle so defined. Of course, no records are kept of the total amount of individual wagers made in the casinos, except in rare instances where

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ing house percentages applicable to the different bets available to the player, and one can give only an average percentage against the players.[5]

In Nevada, the state and local government combined tax on casino gambling is about seven per cent of gross profits. This is much less than Nevada could obtain in taxes; they are kept low, perhaps, because Nevada tries to maximize tourism. We assume that a 75-25 division of gross casino profits would be possible, with the casino operators keeping 75 per cent and the remaining 25 per cent going to governments.

This leads to the question: Who should operate casinos? In New York State, it has been proposed in initial legislation that casinos be operated by the state or by a public-benefit corporation instead of by private operators. There are arguments both ways. In favor of state-operated casinos, it can be said that a greater *percentage* of the profits would ultimately go to the states (but probably smaller *total* revenues), infiltration of criminals into the operation would be much more difficult, corruption in the granting of franchises would be avoided, and policing of operations would be easier. Private franchising, on the other hand, has the advantage of generating more demand and hence revenues, and it avoids having the moral authority of the state eroded by direct association with casino operations.

The experience of Puerto Rico in contrast to that of other Caribbean casinos is instructive on at least the issue of consumer demand. Puerto Rican gambling, closely controlled and supervised, evidently seems to be sterile and non-competitive in contrast with the higher-limit, easy-going operations found, for instance, in the Bahamas. Despite accusations that Bahamian casinos have underworld connections and that cheating sometimes occurs, the customers have not

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particular tables have been "clocked" for some reason. However, each casino keeps elaborate totals of the drop in the game boxes, so that handle defined in this way is a quantity continuously and accurately recorded. In all discussion of "handle" and "hold" and "take" which we have seen, no one ever bothers to make it clear that there are two possible ways of defining and computing these numbers, so that it is never made clear why percentages of "handle" held are sometimes quoted in the 15 to 30 per cent range and other times in the one to five per cent range.

[5] On the crap table, the average wager has about a two per cent advantage to the house; in blackjack, this figure is probably closer to three per cent, whereas in baccarat it is about 1.25 per cent. Finally, the per cent of the *drop* held for each individual game is more or less proportional to the percentage in favor of the house on *individual* wagers. In roulette the figures are 33 per cent for the hold versus five per cent on an individual bet, in blackjack 22 per cent versus about three per cent, and in craps 18 per cent versus about two per cent. There is no hold or drop figure for baccarat because most dealings are in cash from a bankroll on the table.

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been deterred. On the contrary, it appears that the gambling public is attracted to operations spiced by rumors of underworld involvement.

The experience of the Howard Hughes ventures in Nevada is also instructive. A few years back Hughes entered Nevada, purchased six large casinos, and quickly became by far the largest operator in the state. This empire had much of the bureaucratic flavor of a state-run operation in contrast to the smaller, more individualistic operations of the other hotel-casinos. Business in the Hughes casinos fell off, and it has been widely reported that the Hughes organization in Nevada has made no money.

Credit policies will also surely make a difference. Private casinos give large amounts of risky credit for good business reasons. The ability to take the optimum credit risk with each player is a valued characteristic of a casino manager and his staff. It is hard to believe that state-operated casinos (even through the medium of public-benefit corporations) could efficiently run a risky credit operation. Another problem concerns the liberal use of free food, drinks, rooms, and travel—the present *modus operandi* of the Nevada casinos. And there is also the lavish entertainment which is an integral part of private casino operation. Somehow, one cannot envision this sort of Las Vegas-like operation being run by bureaucrats.

Whatever the comparative advantage of private casino operations, the problems of controlling them should not be underestimated. The British experience with casinos is instructive here. Chemin-de-fer was the only legal casino game permitted in England until the 1960 Betting and Gaming Act legalized more extensive casino operations. The number of casinos increased rapidly and by 1968 reached a total of more than 100 in London alone, of which at least 10 were quite large. Unfortunately, there was no clear authority responsible for casinos. As a result, many were owned and controlled by foreigners, violence occurred as control of many small casinos was contested, and protection payments were extracted. When things had clearly gotten out of hand, the government finally passed the Gaming Act of 1968, appointed a Gaming Board, and adopted strict licensing regulations. Almost all the smaller casinos which had caused most of the trouble were forced to close; the larger ones that were allowed to remain open were supervised and forced to remove all foreigners from direct control, tourist gambling "junkets" were forbidden, and all casinos were made to operate as private clubs. Foreigners wishing to join a gambling club must now wait 48 hours before membership is approved, clubs may be open only from 2:00

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p.m. until 4:00 a.m., and casinos with restaurants are now required to route restaurant patrons away from gaming tables.[6] As a consequence of the strict licensing procedures, the number of casinos is much reduced, but the abuses have largely disappeared.

The lesson of the British experience is that too little regulation can cause problems. On the other hand, if an American state decides to have casinos in order to make money and draw tourists, then too much regulation will be counterproductive. States now contemplating having casinos would certainly do well to study carefully both the British and Puerto Rican experiences, as well as that of Nevada, in formulating their own policies.

Bingo. Some states allow bingo for avowed charitable purposes and the handles are already very large. As with other types of gambling, a shift in attitudes has occurred. Many large bingo centers which flourished along with casino gambling in the 1930's and 1940's were closed down by local authorities in the 1950's. For example, in the Ocean Park-Venice and Long Beach areas of California some 15 to 20 bingo parlors (locally called Bridge-O) flourished as late as 1950. Bingo today is permitted in a few of the large Nevada casinos and these operations have usually been quite successful. But now, proposals for removing restrictions are being advanced in a number of states.

Our estimate for the net potential revenue for full legalization of bingo is $500 million a year. Our estimate of the net current revenue from widespread, often technically illegal, but tolerated, bingo games is $100 million.

Card Playing. The main card games which rightfully come under the category of organized gambling are panguinge (or "pan," for short) and the many varieties of poker. Other gambling card games such as gin rummy and pinochle are usually played man to man and do not involve a house take.

In Nevada, about 10 of the casinos have card rooms where poker and pan are played. The revenue is modest but the operators believe that many of the patrons of the card rooms will sooner or later play at nearby casino tables. Outside of Nevada, the only large card rooms operating legally are in California, which has local option for draw poker. The main center is Gardena, in Southern California,

[6] Other restrictions are no advertising, no tipping of croupiers, no liquor served in gambling areas, no gambling in any casino by casino employees, and regulation of odds on games.

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which has five large draw poker establishments. As a result, the property tax rate is very low compared to that of surrounding municipalities.

Our estimate for the present net revenue from illegal organized cards is $50 million; net revenue with legalization of cards, based largely on the Gardena experience, is estimated to be $200 million.

Slot machines. Three (or multiple) reel slot machines, or "one-armed bandits," are tolerated in many localities, although legal in few. In Nevada, each casino has hundreds of such machines available from denominations as low as one cent per play to a high of $5, and it is not unusual for a large Las Vegas casino to gross $7,000 or more a day from its machines. The slots may be designed to return on the average any desired percentage of the amount played to the customer. In the large casinos, a common figure is about an 85 per cent return to the players. It is not unusual in supermarkets, drug stores, and similar locations for slot machines to have a "hold" of 50 per cent or even more.

Our estimate for the present net revenue from illegal slot machine operations is $70 million a year. If they were legalized and made widely available, the estimated annual revenue to the states is $500 million. There are also other coin-operated games, legal and illegal, but the amounts of money involved in them are comparatively small.

Full legalization: the potential revenues

Present reported revenues from legal gambling, our estimates of the net revenues from current illegal operations, and the potential from nationwide legalization are summarized in Tables 1, 2, and 3. Currently the nationwide gambling handle is about $30 billion a year, one-third legal and two-thirds illegal. The states get a little under $1 billion in revenues while the illegal operations net less than $2 billion. With wide-open, nationwide legal gambling, our estimate is that the total handle would be about $100 billion and, assuming a high tax rate, that the total revenues to the states would be about $7 billion.

There are, of course, several qualifications and uncertainties about these numbers. The assumption that all the major types of games will be legalized in every state is clearly an overestimate, since it is doubtful that even 10 years from now there will be complete legalization in half of the states. Many states, especially in the Midwest,

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TABLE 1. *Present Reported Legal Gambling Revenues for Major Games*

	HANDLE	GROSS TAKE	GOVERNMENT REVENUE
	(IN BILLIONS OF 1973 DOLLARS)		
Racing (on-track)	5.50	1.00	.50
Off-Track Betting	.55	.10	.06
Lotteries	.50	.30	.20
Casinos	3.60	.70	.04
Total	10.15	2.10	.80

TABLE 2. *Present Estimated Illegal Gambling Revenues*

	HANDLE	GROSS TAKE	OPERATORS NET REVENUES
	(IN BILLIONS OF 1973 DOLLARS)		
Off-Track Betting	5.50	1.00	.50
Lotteries	.20	.10	.05
Numbers	2.50	1.00	.25
Casinos	2.00	.40	.15
Sports Betting	12.00	.70	.50
Bingo[1]	.30	.15	.10
Cards[2]	—	.07	.05
Slot Machines[2]	.40	.10	.07
Total	22.90	3.52	1.67

1 Excluding games operated by churches or charities.
2 Other than in casinos.

TABLE 3. *Future Potential Legal Gambling Revenues*[1]

	HANDLE	GROSS TAKE	GOVERNMENT REVENUES
	(IN BILLIONS OF 1973 DOLLARS)		
Racing (on-track)	11.00	2.00	1.0
Off-Track Betting	11.00	2.00	1.0
Lotteries	2.50	1.20	1.0
Numbers	4.00	1.00	.5
Casinos	36.00	7.20	1.8
Sports Betting	33.00	2.00	1.0
Bingo[2]	2.00	1.00	.5
Cards[3]	—	.80	.2
Slot Machines[3]	4.00	1.00	.5
Total	103.50	18.2	7.5

1 There might also be a sizable amount of illegal gambling in the future.
2 Excluding games operated by churches or charities.
3 Other than in casinos.

may be slow to legalize any sort of gambling. (These "prohibitionist" states, however, will not have a large proportion of the nation's population.) Moreover, some kinds of gambling will not become uniformly available even where permitted by state laws, but will be concentrated in particular centers. This will be especially true of casinos, which are likely to be established initially in resort areas.

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It will probably be a long time before gambling casinos appear in Manhattan.[7]

In addition, there is a great deal of uncertainty as to how the various types of gambling will interact with one another. On the one hand, competition among the various forms of gambling may increase the revenues to some types at the expense of others; this could make some of our individual estimates and the total too large. On the other hand, widespread legalizing might lead to reinforcement. Many people who had never gambled before might become addicted players. If this were to happen, then our estimates could turn out to be too low. Therefore, we put an uncertainty factor of about two on these estimates. If the negative factors prevail, our estimates may be high by a factor of two; if the positive factors prevail, then they could be low by a factor of two. This uncertainty factor of two is only a slightly educated guess. It might be, say, one and a half or three, but almost surely not 10.

In any case, we emphasize that these projections assume that gambling has been widely legalized and that the industry has matured. Within the next decade, perhaps half of the expected potential might be realized.

The projected $18 billion gross take shown in Table 3 would be the sum lost by gamblers to operators. Out of this total would come the costs of running the gambling enterprises, the return on investment, and the amounts retained by or transferred to governments. To put these figures in perspective, the losses to players if gambling on this scale had occurred in 1973 would have come to about two per cent of total personal income. (Our estimates are that losses actually were about 0.6 per cent.) This is about twice the share of personal income spent on gambling in Great Britain. But if Britain is a "nation of gamblers," as it has been called, how can our projection of gambling at twice the British level be justified? First, lotteries, as such, are illegal in England and they contribute much of our estimate. Casino gambling in England, now greatly restricted, is far below the level we project for the United States. Moreover, sports betting on individual games is very small in England compared even to our present illegal levels, let alone to our projected legal levels. In short, we are assuming a greater variety of games with a wider appeal than exists in England. Second, the British are poorer than we are, which means that we have more discretionary

[7] As this article goes to press, a group of mid-town Manhattan businessmen have proposed legalizing casino-restaurants in the Times Square section with the stated objective of rehabilitating the area.

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income available for entertainment. In effect, we are assuming that gambling expenditures increase more than proportionately as per capita national income increases.

Here again we face the uncertainty in our estimates. If England represents a saturated gambling condition, and if the higher income elasticity argument doesn't hold water, then our estimates for the United States could well be too large. But if all forms of gambling are legalized, and if our high standard of living permits proportionally larger gambling expenditures, and if this country becomes truly a "nation of gamblers," then our ultimate projections could be too low.

Illegal gambling: How much is there?

The estimates for current revenues from illegal gambling shown in Table 2 are a handle of about $20 billion and a gross take of $3.5 billion. The gross take is the total amount lost by players. Out of it come payoffs to law enforcement personnel (we estimate that these payoffs total roughly $500 million a year). The gross take also covers the salaries and other costs of running a large number of illegal organizations. Operators net take, estimated at about $1.7 billion, is the sum retained as profits by illegal operators after expenses.

Our estimates of illegal gambling contrast sharply with other published estimates, which have almost all been considerably higher, ranging up to 20 times as much. There appear to be two distinct causes for these inflated estimates. One is the motive of arousing public attention to the dangers of illegal gambling: the higher the estimate, the more menacing the threat. The second is confusion among the terms "handle," "gross take," and "net take." For example, John Scarne (in *Scarne's Complete Guide to Gambling*) estimates an annual betting handle of $500 billion. He claims, however, that only half of this is bet through the medium of organized gambling, where there is such a category as "house take"; the other half is private gambling. Scarne also uses the definition of "handle" in casino gambling that counts every bet made rather than the much more useful definition that equates handle to drop. The difference exaggerates the estimated handle by a factor of about seven.

Another mistake is to confuse handle with either gross or net revenue. An instance of this occurred in the *Advocates* television program on gambling aired in January 1972. The moderator, Mr. Dukakis, began by saying that illegal gambling in the United States

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amounted to at least $20 billion, which seemed to refer to the handle. The next panel member to speak, Mr. Jack Cole, then said that the underworld "rakes off" an estimated $50 billion a year from the action, which seemed to refer to gross take.

Confusion may be understandable on a television show, but many writings exhibit similar misunderstandings. Rufus King, in a 1967 study prepared for the President's Commission on Law Enforcement, concluded that the gross handle of illegal gambling is not less than $20 billion a year, an estimate similar to ours. He then stated that "of this, the annual 'take' of the gambling promoters—i.e., the profits for organized crime—is $6 to $7 billion per year. And out of the latter, some $2 billion per year finds its way directly and indirectly into the hands of corrupt public officials and law enforcers."

There are several difficulties here. First, the annual "take" of $6 to $7 billion would be one third of the handle. But in sports betting, which is a large part of the total, the take is only about six per cent, and in casino gambling the take is about 20 per cent of the (properly defined) handle. Second, King goes on to confuse the gross take with the net take by saying that the annual take is the same as the profits for organized crime.

Exaggerated and confused estimates have also been set forth by more authoritative sources. The President's Commission on Law Enforcement in 1967 repeated King's estimate of a profit of $6 to $7 billion per year. President Nixon's 1969 message to the Congress on organized crime exhibited the familiar confusion: "Estimates of the take from illegal gambling alone in the United States run anywhere from 20-50 billion dollars, a figure larger than the entire federal administrative budget for the fiscal year 1951." Once again, it appears that "handle" has been converted into "take."

Gambling revenues and state budgets

Does the existence of a substantially smaller scale of illegal gambling than has widely been assumed make any difference? For one thing, it indicates that although illegal gambling may be a significant problem in certain communities, its seriousness as a nationwide problem may have been exaggerated. For another, it suggests that if the diagnoses of so many alleged experts are as faulty as they appear to be, their remedies might also be questioned, whether they advocate more rigorous law enforcement, or opt for wholesale legalization.

If the profits from illegal gambling are inflated, so too are ex-

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pected government revenues from its legalization. Today Nevada gets about 40 per cent of its state-level revenue from gambling taxes, New York State about three per cent, and New Jersey about five per cent. In the not too distant past, several states got much larger proportions of their revenues from gambling. For example, in 1946 New Hampshire and Rhode Island each obtained 16 per cent of their revenues from the tracks. But that was back when the states had fewer responsibilities and very much smaller budgets than they do today. And there was less competition. Perhaps one fourth of the New Jersey lottery sales and a majority of New Hampshire's sales have been to non-residents, and with the spread of legal gambling their non-resident trade is shrinking.

In order to get a sense of what gambling revenues might mean for all of the states together, assume that the amount of gambling we estimate would occur in a fully legalized nationwide system were taking place today. Our projected gambling revenues of $7.5 billion in 1973 dollars come to 2.0 per cent of total taxes collected by all levels of government, and 5.5 per cent of total state and local revenues. This amounts to about 25 per cent of the yield from property taxes, and is about equal to the combined total of state gasoline and cigarette taxes, or to the present level of federal revenue sharing.

For some states, of course, the share of gambling taxes would be much higher than this average, and a higher share would mean dependence. But dependence on gambling revenues would create a conflict with other state objectives, including that of regulation to avoid abuses. Where revenues conflict with regulation, the incentive to subordinate the regulatory function is strong and may be overwhelming. It is foolish for a state government to believe that it can rigorously control gambling enterprises to protect the public welfare if doing so will result in a clear and negative impact on the public purse. The fact that the administration of lotteries in several states is lodged in state finance departments suggests how this conflict between regulation and revenue is likely to be resolved.

In the view of the Bank of New South Wales, this conflict has been evident in Australia: "The states have become more involved in direct operations of lotteries and dependent on the maintenance of legal betting. They have therefore acquired a vested interest in stimulating gambling as a means of raising revenues." The same phenomenon has been noted with respect to the Italian state lottery.

Clearly, $7 billion is not a trivial sum. Though it is not nearly

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enough to solve the revenue problems facing the states, it may well be enough to get some of them hooked on gambling.

Gambling and organized crime

Not only are the profits from illegal gambling almost certainly less than the huge sums usually cited, there is also doubt about the relationship between illegal gambling and "organized crime"—and, indeed, about just what is meant by "organized crime." Most of the discussions on this subject suggest the following conclusions:

1) All of the profits from illegal gambling go to "organized crime."

2) Organized crime is synonymous with the "syndicate" or the "Mob" or the "Mafia" or "La Cosa Nostra," which operates on a nationwide scale.

3) This "organized crime" which controls illegal gambling is the same organization that controls narcotics, extortion, prostitution, some labor unions, loan sharking, and other nefarious operations.

Although there is no question about the existence of criminal organizations and of links of some kind among some of them, the evidence in support of propositions as sweeping as these is far from convincing. The publicly available evidence is consistent with a pattern of local control of illegal gambling (i.e., illegal gambling in Los Angeles is, by and large, run and controlled by local Los Angeles people, whereas illegal gambling in New York is controlled and run largely by New Yorkers). This pattern, in fact, seems to prevail. The ties among these local organizations seem to be weak at best and often non-existent. Moreover, although there is certainly some interconnection between illegal gambling and the other types of crime mentioned above, a high degree of overlap has not been established. It is a more plausible conjecture that some operators engage only in gambling while others also engage in other criminal activities. We estimate that gambling proceeds are divided about evenly between these groups. And of the group that runs both gambling and other illegal activities, only some are likely to have ties to "organized crime." Again, perhaps the proportions are about equal.

An approximate check on the degree of involvement of "organized crime" in gambling can be made by calculating the per capita incomes implied by various estimates. The FBI has estimated that "La Cosa Nostra" or the "Mafia" has a membership of about 6,000. If all gambling profits went to its members, the per capita amounts

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involved, using both the more or less "official" profit estimates of $6 billion a year and ours of $1.7 billion, yield $1 million and $300,000 a year per member respectively from gambling profits alone. Although millionaires have undoubtedly been made from the profits of crime, it is obvious that the average income of members of the Mafia is not this large. Certainly, our conjecture on the pattern of gambling control yields a more plausible, although probably still too high, estimate of gambling income to members of the "Mafia."

The acceptance of a lower estimate of illegal gambling profits and corruption payments should also affect arguments for legislation based on the elimination of this source of corruption. The Knapp Commission report has been cited above. Another report, issued in 1972 by the Committee for Economic Development (CED), urged that all laws against casual gambling be repealed and that all organized gambling be taken over by the government. The report argued that "such action would erode the corruptive power of organized crime, now heavily fortified by illicit gambling profits. And it would provide a sizable source of public revenue."

The wholesale legalizing of gambling in order to lessen police corruption is a drastic remedy. The situation in New York City may justify the Knapp Commission's position, but we doubt that either it or the CED group was in a position to forecast the consequences of legalization. A more careful and selective approach to legalizing gambling in New York, and *a fortiori* elsewhere in the country, seems prudent.

The case for trying to eliminate illegal gambling through legalization rests on several propositions that, at the least, require examination. One argument often advanced is that profits from gambling flow into other criminal activities such as loan sharking or narcotics distribution. This proposition seems to rest on the premise that these other illicit activities need permanent subsidies or that they incur large "start-up" costs. To be sure, some illegal operations are, in effect, conglomerates that no doubt derive strength from having interlinked activities, but the notion that gambling provides essential support to other criminal activities that would seem to be capable of generating profits on their own seems far-fetched as a general proposition. Indeed, the possibility must be considered that the cutting off of illegal gambling funds will cause some elements of "organized crime" to *intensify* their activities in narcotics and other illicit trades or to search for other kinds of illegal activities to enter. If illegal organizations were profit maximizers, they would

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already have developed these other activities. But a model which attributes a degree of organizational slack to illicit firms suggests that the erosion of gambling profits would lead to a search for other sources. It is not obvious which model is the more appropriate one, but it might be excessively optimistic to assume that reductions in illegal gambling would result in *no* increase or even a decrease in other kinds of crime.

Finally, it is true that if the community is willing to go far enough in legalizing gambling, imposing low taxes on it, and enforcing the laws, the market share of illegal gambling operators will certainly fall. But it should be noted that a policy of imposing very low taxes on legal gambling in order to compete more effectively with illegal operations is not consistent with a policy of maximizing revenues. Moreover, based on our figures, the illegal market share could be reduced from two thirds of the present total ($20 billion out of a $30-billion national handle) to a smaller share of a much larger total, and still be absolutely quite large.

Who gambles?

One view of gambling is that it is a useless activity, a luxury, and ought to be subject to taxation. This was the view of the British Select Committee on Betting Duty in 1924:

> Your committee are unanimous in the view that betting is a useless thing except insofar as it gives pleasurable excitement for some and for others an escape from the monotony of their daily world. It is in the main unproductive except to the bookmakers. In others, it is not inaptly described as a "mugs game," as practiced by the great majority of backers. It is a foolish occupation or habit. It is a pure luxury and a fitting subject for taxation unless there are strong reasons for allowing luxury to escape.

On this view, gambling taxes can be avoided by not gambling. Nevertheless, if the poor are more likely to gamble than the rich, the operational effect of taxing gambling would be to transfer income from the poor to those better off. More generally, if this country is going to experience a major growth in gambling, we need to know much more than we do about who gambles, how much they lose, and the probable incidence of gambling addiction. And we also should know more about what happens to the gambling propensity of a community over time as incomes increase. For a given availability of gambling opportunities, should we expect to see proportionally more, less, or the same amount of gambling over the next

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20 years? Or say by 1984? George Orwell's forecast in his famous novel was this:

> Heavy physical work, the care of home and children, films, football, beer, and above all, gambling filled up the horizon of their minds. The lottery, with its enormous weekly prizes, was the one public event to which the proles paid serious attention and the principal thing for which some of them lived. It was their delight, their anodyne, their intellectual stimulation.

But what is known? As with much about gambling, very little, although there seem to be better data in Britain than in the United States. A Social Survey in the United Kingdom in 1950 and several surveys since then have shown that the incidence of gambling is highest in low-income groups. For example, the frequency of betting on horses and on football pools is much greater in the working class than in the upper and middle classes. The principal exception to this pattern seems to be casino gambling, because the casinos have been designed to appeal to the affluent.

Some evidence on differences in the kinds of bets made by different socio-economic groups has been provided by the British publication *Sporting Life,* which showed that a shop that served the lower-class areas and that handled smaller bets had, on the average, both higher costs and higher profits. For each pound bet, the poor bettors got a worse return than did the upper-income bettors. The big bets tend to be at short odds and on horses more likely to win, but most bets in working-class areas are on long shots or combinations, on which the operator has a larger takeout. These are, in fact, the only rational bets for the poor to make, for only these long-odds bets offer them any chance of winning a really significant amount of money.

Within the United States, early surveys taken for the New York City Off-Track Betting Corporation showed that its customers were similar to the general population but with a bias toward above-average income and education. Its most frequent bettors (those placing 100 or more bets a month with OTB), however, were below average in income and education (24 per cent had incomes below $7000). If we take account of amounts bet, it appears that OTB's handle is biased somewhat towards the low end of the income distribution. A similar inference can be drawn from a survey of New York State lottery ticket purchasers made in 1969. Evidently, the willingness to wager a small amount was actually less at high incomes than at lower ones. The Quayle survey in New York showed gambling on numbers to be concentrated in middle-income and low-income black

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and Puerto Rican groups, although whites were also substantially represented. The survey also showed that heavy sports betting with bookies is disproportionately concentrated in the middle-income group.

For the United States as a whole we know little about the incidence of gambling as a function of people's incomes. We would expect to find that the incidence of gambling varies with different types of games, and this is suggested by the evidence. But the data on distribution we have seen are too sketchy to be of much use for the purpose of determining policy.

Addiction and corruption

We can, however, say within the confidence limits stated above that gamblers currently lose something over $5 billion a year and that legalization might see such losses tripled. But how should one interpret a projection of a possible $100-billion gambling handle? As we have seen, the handle is an index of the total number of gambling transactions (i.e., the amount of gambling), and thus can be interpreted as a measure of the preoccupation of people with gambling and the amount of time they spend doing it. On this view, it will be an important aspect of many people's lives. This preoccupation is likely to be manifested in two specific and troubling ways: gambling addiction and an altered community atmosphere.

Although little is known about the incidence of gambling addiction, it can be a serious problem. Concern about compulsive gambling was expressed by the Gaming Board in Britain in its 1969 Report:

> The major objection however to the use of entertainment and dancing as a means of attracting people to take part in gaming must be that it increases the number of people who acquire a taste for this activity. Some proportion of them will eventually be compulsive gamblers. It is not at present possible to form a view as to what that proportion is likely to be—the information on which such a view must be based is not available. Nor is it possible to predict what proportion of compulsive gamblers may eventually achieve a cure. It is frequently argued that the innocent enjoyment of the majority should not be interfered with for the sake of a small minority whose weakness leads them into difficulties. For want of information, this argument must be broadly accepted, but with important reservations. It may well be less expensive in the long run (both in money and in terms of human suffering) to limit somewhat the opportunities for people to be led into gaming in the first place, than to attempt to cure those who subsequently develop compulsive cravings. It would seem particularly important that oppor-

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tunities to acquire a taste for gaming should not be made too readily available to young people.

The line between meeting a legitimate demand for gaming and going too far can be a fine one. Landau, the former head of the Israeli lottery, believed that weekly drawings would sell better than fortnightly ones and so advocated them, but he felt that drawings every two days were an indication of a sick society.

The importance of learning more about the incidence of gambling addiction and the conditions in which it develops is suggested by analogy with drug addiction. For many decades, drug addiction was thought to be a minor social problem affecting Chinese immigrants, jazz musicians, and a few other special groups, but now it has emerged as a prominent national concern. Gambling addiction is evidently not a major problem today, but if we reflect on how little we know about the psychology of addiction, it might make us consider the possibility that it could develop into a serious social ailment.

The effect of gambling activities on the general atmosphere of the community will also be of wide concern. Many who do not disapprove of gambling in general will oppose it in their own community. It is especially troublesome when the state uses its power to force gambling establishments on communities that do not want them. For example, objections to having OTB outlets have occurred in some neighborhoods in New York. The usual way this concern has been dealt with is by segregating gambling activities in resort towns and in separate jurisdictions (Gardena serving the Los Angeles area, Covington in Kentucky serving Cincinnati—years ago but no longer—and, most importantly, Nevada serving the United States as a whole).

Zoning of gambling has a good deal of merit. It has been applied strictly by European countries, which have usually restricted casinos to watering places, kept them out of major cities, and banned local residents from entry. Although restrictions of this severity would not be acceptable in the United States today, this approach has much to commend it. Localities willing to accept gambling can get the financial benefits while the residents of places unwilling to live intimately with it can travel moderate distances to do their gambling. People who do not find themselves in the desired type of community will move to the other. However, the desire of legislators for revenue, or businessmen for trade, or libertarians for removing constraints may prevent a sensible pattern along these lines from emerging.

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How rational is gambling?

Adam Smith viewed gamblers as conceited men who believe that their fortune is greater than that of their neighbors and who overrate their chances of gain and underrate their chances of loss. But if one considers the near universality of gambling from earliest recorded history, one wonders about this view. Moreover, an examination of gambling behavior in different countries today shows some interesting regularities. For example, in a number of countries for which data on legal gambling are available, net expenditures on gambling seem to fall within the range of 0.4 per cent to 1.0 per cent of national income.

Various attempts have been made to account for gambling behavior. Some scholars have developed the "pleasurable excitement" and "escape from monotony" themes of the 1924 British Commission. Others stress the fact that many people believe life is dominated by chance and they just might get lucky. A third group focuses upon the skill and computational activities involved. Still others emphasize the importance of social group interactions that often occur in the gambling environment. Psychiatrists have suggested that gambling behavior increases sublimation of oedipal aggression against one's father, that it is a substitute for masturbation, that it is a defense against feminine identification which takes the form of excessive masculinity, and that bettors have an unconscious wish to lose. It seems plausible that several of these motivations may play a role, but there is much to be said for economizing on explanations.

Perhaps the simplest is that people gamble for money. This explanation may seem paradoxical, because on the average they lose. For example, a 50-cent lottery ticket has an actuarial value of around 25 cents. Milton Friedman and L. J. Savage have shown, however, that this kind of purchase can be explained by assuming that the gambler has a particular utility preference for money: Over a range of income levels, he has an increasing marginal utility for money—in contrast to the usual assumption that the marginal utility of money decreases monotonically as wealth increases. It isn't necessary to postulate a preference for thrills or risks. All a person needs is a desire for a large sum of money. This prospect is precisely what a lottery offers.

If lottery-like games can be explained with some economy of means, what about games that offer lesser odds (in the 100-to-1 to 1000-to-1 region), like numbers or combination bets such as Exactas? For people who play these games, saving would be a more reliable

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way of amassing some money. But suppose we assume a life beset with crises, a life that makes saving difficult or impossible, and also assume a high personal time rate of discount and a limited ability to borrow money (and then only at a high rate of interest). For such people, it may not be irrational to play unfair games, even though the mathematical expectation is negative. Such games might seem to offer the only hope of accumulating a lump sum.

It is only when one gets to short-odds games, played repetitively, where money is put right back into the game, that we must resort to "pleasurable excitement" or to the various other kinds of non-economic explanations.

Motivations for gambling are important to understand because they provide insights useful for public policy choices. For example, almost everybody who bets on a lottery loses. This seems to be why individual outlays on lotteries are small and lottery playing is non-addictive. Consequently, the long-odds "lottery" type of market probably can be satisfied in a number of ways. Competition among a well-run lottery, numbers, and combination bets of various kinds may be keen. Increases in the take from one of these games are likely to be at the expense of the others. For the shorter-odds types of games—especially those where repeated, rapid play is possible, as in casinos—the commodity being sold is a more complicated "package." The psychological involvement is more addiction-causing. Moreover, these types of games are probably less close substitutes for one another and hence less strongly competitive. This is good from the point of view of the revenue maximizer because the market can be partitioned and developed, but the social consequences of widespread availability of this class of games may be serious.

The federal role

The federal government has generally been hostile to gambling, legal or illegal. It has imposed a 10 per cent tax on most forms of gambling, exempting only casino games and money wagered in pari-mutuel pools and state lotteries where drawings are based on horse race results. It prohibits the transmission of lottery tickets across state lines and forbids banks to sell them. The FCC forbids the broadcasting of lottery drawing results. Gambling winnings are taxed, in contrast to the practice in many countries. Bookmakers in Nevada are prohibited from accepting out-of-state bets by telephone and are burdened with the 10 per cent tax.

If legal gambling expands, it seems unlikely that the federal role

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can long remain what it is today. Increased competition among states is likely to lead to a demand for a stronger federal role. On this question, the history of competition among the states in Australia is instructive. In 1902, the central government refused to accept mail for Tasmania's lottery because other states felt that money was being drained from them. Addresses of convenience and agents were therefore used to sustain sales. In 1931, New South Wales introduced a lottery, in part to keep money from going to Tasmania and Queensland. In 1954, Victoria persuaded Tattersall's lottery to move there from Tasmania because it was attracting too much money away from Victoria. Tasmania responded by licensing another operator who provided bigger prizes than Tattersall's. The main effect of all this competition was to increase the stakes, reduce the interval between ticket sales and drawings, and produce more frequent drawings—a process we now observe at work in the United States.

The federal government in the United States will also be under pressure from legal gambling interests to remove obstacles such as the taxation of gambling winnings and the 10 per cent betting tax. Exempting winnings would certainly help shift action away from illegal operations. It also would help expand the total amount of gambling.

Weighing the alternatives

State legislators have several factors to weigh in choices on gambling. Arguments in favor of legalization are increased tax revenues, reduced corruption, increased tourism, and removal of restrictions on personal choice. On the other hand, general legalization will increase substantially the amount of gambling, and this will have uncertain but probably significant consequences on addiction and community atmosphere. Gambling taxes will not do much to ease the burden of taxes in most states; and in the few states where these taxes might reach a substantial level, they will create a relationship of dependency. Moreover, legislators should be aware of the conflict between the function of government as regulator and its possible role as operator; there is much to be said for limiting government's role to that of regulator. States that permit widespread gambling will also want to consider seriously the possibilities of limiting the "spillover" effects by adopting strict zoning provisions.

On the fundamental issue of legalization, the states are faced with three broad options:

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1. *No change.* This would mean foregoing comparatively small tax revenues and living with existing illegal gambling and corruption, which in many states is not a serious problem. This policy would avoid the social costs associated with increased gambling. And if neighboring states offer gambling services, the abstaining state's citizens will not be terribly deprived. No doubt some states will prefer this alternative for at least the next decade.

2. *Selective legalization.* This would mean legalizing only the most easily controlled forms of gambling. States that choose this alternative would accept on-track betting, lotteries, and perhaps numbers (in states that have large illegal numbers business) and, possibly, off-track pari-mutuel betting on horses. Another possibility is sports betting on a pool or pari-mutuel basis of the British type. Casinos, sports betting on individual games, card parlors, and slot machines would be excluded. A difficult problem these states will face is deciding where to draw the line.

3. *General legalization.* Some states may legalize virtually all types of gambling. They will then have to live with the various consequences of that decision.

Our findings are not sufficiently certain to predict what will happen with confidence; still less do they enable us to say what a state should do. But our analysis does suggest the existence of illusions and misconceptions about the consequences of legalized gambling, and the importance of looking closely at its possible impact. No one knows what the long-term effects of legalizing gambling would be. Yet we can find out a good deal by examining our experience to date with illegal and legal gambling. The variation in gambling patterns across states and over time provides a wealth of data, and further analysis of British data would also be very useful. Some research has begun and even these limited results are helpful. But much more needs to be done.

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Accountability in education – the Michigan experience

JEROME T. MURPHY & DAVID K. COHEN

FASHIONS in social reform shift quickly in the United States and education is no exception. The first wave of reform in the 1960's assumed that inequality in education was a prime cause of poverty. Programs sought to rectify this by changing the allocation of school resources and the assignment of students. Job training, remedial education, and desegregation were prominent weapons in the Great Society's arsenal.

By the decade's end, however, nearly everyone suspected something was amiss. Activists observed that infusions of money seemed to produce little change in schools—if, indeed, the money got there at all. Researchers noted that remedial education and job training produced no large or consistent impact. Advocates of reform found that schools had a seemingly endless ability to absorb pressure for change, all the while changing not a whit. For different reasons, the sense began to grow that redistribution was not enough—that greater control over decisions was required.

This was the occasion for a second wave of reform ideas, centered on the notion of *accountability*. Like most metaphors concerning reform, this one covered a variety of disparate elements: performance contracting, community control, management-by-objectives, educa-

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tional vouchers, management information systems, and educational assessment.

This is a pretty diverse assortment of ideas, and there is an equally diverse collection of political types marching together under the banner of accountability. Fiscal conservatives approve because they think better management will reduce costs. Community organizers like accountability because they think direct political control will promote change. Up-to-date bureaucrats and academics like it because they have been taught to believe that clear ideas are important. Reformers in the foundations and professions like it because better information and precise goals seem appealingly uncontroversial vehicles for reform—a pleasing notion after the turmoil of the late Great Society.

These views of accountability have little in common save the idea that bureaucracies and professions have gotten out of control and need to be disciplined. But they can be divided into two rough clumps. One contains those schemes which assume that if government can get more rational information and use it in more scientific ways, the schools' performance can be improved. The other assumes that the schools' problems are less scientific than political, and that clients and constituents should influence educational decisions more directly.

In the last four or five years there have been a few efforts to promote this second approach, which seeks to provide accountability by political means. The experiments with community control and tuition vouchers, however, have been few, far between, and full of political dynamite. Meanwhile, American education has been having quite a romance with technocratic approaches to accountability. Seeking better information seems less threatening and more up to date than redistributing political power. Two major foundations and the U.S. Office of Education have supported a National Assessment of Educational Progress, and more than 30 states have already adopted some variant of this idea. A large number of state and local school systems have implemented or experimented with various programs to improve management information. And quite a variety of schemes for specifying educational objectives have been tried at the state and local level. In practice, accountability has come to mean reforming government through science.

The fate of such reforms is intrinsically interesting for a society in which science is a major source of authority. Particularly noteworthy is the reform's assumption that democratic politics will work better if action is based on scientific information. The idea is that

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political control has at least partly failed in education because of vested interests, popular apathy, the complexity of modern society, or other reasons. Reformers assert that administrative action will be more effective if scientific data are substituted for—or added to—political intelligence.

But these accountability schemes must operate in a distinctly nontechnocratic political environment. American schools are formally responsible to the electorate, through (mostly) elected boards at the local level and elected representatives at the state level; much of their management rests in the hands of professional teachers and administrators; and there is by now a well-established and stable system of interest groups which influence both state and local school affairs. In this environment, does better information improve administrative control, or reduce conflict? Does it clarify decisions?

We pursued these queries in Michigan, which has had a rather extensive experience with accountability. Reform there has been carried out at the initiative of the state government. One effort involved a statewide assessment program, designed to improve decision making by collecting and publishing information on the performance of students and schools. Another sought to reduce school failure for educationally disadvantaged children by means of performance incentives that conditioned extra school aid on learning gains made by these children. The following account will not fully answer our questions, but it will provide some indications of the way accountability works and some suggestions about the relation between scientific and political control in education.[1]

I. Assessment

In Michigan, assessment meant different things to different people. State education agency officials saw it as a way to achieve several reforms. One was to help the poor by collecting evidence on unequal school resources and results. Another was to make resource allocation and decision making more rational, by improving the information base for decisions. A third was to loosen up a rather closed system of professional dominance in school decisions, by making a great deal

[1] This essay is based on more than 50 interviews conducted in Michigan in January and February of 1974. State agency leaders, legislators, executive branch officials, legislative aides, and state lobbyists were the main source of information at the state level. In addition, we visited three school districts and five schools, interviewing school superintendents, federal and state program coordinators, principals, and teachers. Also, we examined all the available written material on Michigan accountability.

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of information public. Michigan taxpayers' groups, however, viewed assessment as a way for the public to find out what the schools were producing for their tax dollars.

Several analysts who had joined the state agency in the late 1960's to staff a new research office were struck by the "lack of reliable information about the level, distribution, and progress of education in the state." One of them, Philip Kearney, had just completed a staff assignment involving a major study of Michigan school finance, and was painfully aware that children from poor families didn't have the same level of school services as wealthy children. The researchers thought better information would be useful in efforts to equalize educational opportunities because they expected assessment would show that school expenditures were highly correlated with student achievement. They could then argue that aid for poorer districts would raise achievement for low-scoring students.

The state reformers also hoped to rationalize educational decision making. In this they were greatly influenced by Ralph Tyler, who had been leading the drive for a national assessment. They quoted Tyler on the reasons for assessment:

> *In making wise decisions, dependable information about the progress of education is essential;* . . . We have reports on numbers of schools, buildings, teachers, and pupils; we have data on the monies expended; but we lack sound and adequate information on educational results. Because dependable data are not available, the public relies on personal view, distorted reports, and journalistic impressions in forming its opinion, *and the schools are both frequently attacked and frequently defended on the basis of inadequate evidence. Only a careful, consistent effort to obtain valid data about the progress of American education will correct this situation* (emphasis added).

With Tyler, the Michigan reformers assumed that objective information on student performance could lead to wiser decisions. This view rested upon several key assumptions: first, that quantifiable relationships existed between school inputs and student achievement; second, that the schools had common goals which could be identified and agreed upon; and third, that there were acceptable techniques for measuring achievement of these goals. All these assumptions were later to be challenged.

These reform ideas were not the only motives behind assessment. In the late 1960's the educational climate in Michigan was becoming more conservative. Escalating school costs, short revenues, higher taxes, racial conflict, and failing students were eroding confidence in the schools and encouraging taxpayer demands for hard evidence of

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educational results. These dissatisfied taxpayers were not concerned with reallocating funds to the poor, but with making schools more effective and efficient. A 1969 report of the Citizens Research Council of Michigan (a creature of Michigan business interests) reflected this point of view:

> Most citizens of Michigan and other states want quality education and *are willing to support effective educational programs.* However, increasingly they are asking for evidence of progress in education as a result of increasing expenditures (emphasis added).

The inference seems to be that funding should be continued only for those programs that are "working." Like the state's researchers, this group believed that better information would foster improved education. It also assumed that the technology existed to evaluate the schools and to distinguish effective from ineffective programs.

So for a number of different reasons Michigan was ready in the late 1960's for some kind of assessment program. Nobody was quite sure exactly how it would work but everyone agreed that more information was needed. The program's implementation, however, not only failed to enlarge this initial area of agreement, but resulted in increasingly bitter conflict. And in the process, it called into question the underlying assumptions of the whole assessment effort.

Assessment becomes law

Michigan's assessment program found its way into the statute books as a result of the interplay of a variety of political forces. In January 1969, the State Board of Education, under growing pressure from taxpayers, decided it wanted to play a greater role in improving and regulating the quality of education. The Board advanced the idea of increased state involvement in school accreditation, a function then performed by the University of Michigan. The (then) state superintendent, Ira Polley, opposed the idea, and instead offered to carry out the assessment Kearney and his colleagues had been considering. The Board accepted, but it balked at the staff's desire for three years of planning to develop a comprehensive program. The Board wanted immediate results.

This feeling was stimulated by Governor Milliken, who formally entered the act while the Board was considering assessment. He addressed a special message on education to the legislature, which at the time was weighing a school evaluation program of its own. Milliken said that it would be irresponsible to pour more funds into

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education "until we can assure taxpayers they will be getting full educational value for their money." Faced with these pressures, the Board at its next meeting adopted an assessment program that involved immediate collection of evidence on school resources and achievement. This proposal was adopted by the legislature, after some urging from Superintendent Polley. A line item was added to the Department's budget bill that year, and the Board's assessment program became law with little fanfare in August of 1969.

In January of 1970, a bare five months after funds were appropriated, the first assessment was set to be administered. Tests in reading, English expression, vocabulary, and arithmetic computation were to be given to every fourth and seventh grader. Data also would be gathered on student attitudes, aspirations, and social and economic backgrounds, on school district financial resources, and on school district staff resources.

But before the tests could be administered statewide, they set off a series of political explosions. Bewildered state officials were first attacked by their expected allies—civil rights leaders and parents of underachieving students—who complained bitterly about racism and invasion of privacy in the collection of background data through student questionnaires. (Students were asked not only about their parents' education but also whether their families had a vacuum cleaner, a dishwashing machine, a dictionary, and so forth.) Parents in Detroit threatened to boycott the tests. The NAACP supported the parents' action, and the Michigan Civil Liberties Union considered suing for an injunction to halt the program. Then the same officials fell victim to an even greater public furor over several reading comprehension questions. One passage began: "Let me tell you about the very rich. They are different from you and me." Conservatives saw this as "Communist propaganda," "anti-free enterprise," and "anti-American." The Department of Education had to do some fancy footwork to stop the legislature from scuttling the new program before it even got off the ground. But the furor did recede, and local schools reluctantly administered the tests and forwarded the results to the Department.

The data: secret or public?

Having collected a truckload of data, state officials set about the analysis. While the results did provide census-like information on student achievement, to everyone's dismay the preliminary findings did not show strong relationships between school achievement and school resources. This was a blow to the agency reformers, for it meant

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that the assessment would not provide quick scientific support for reallocating school aid to the poor. Hoping that careful correlational analysis would produce more impressive results, the state promised to publish its findings later, when the analysis was complete. As it turned out, these efforts still turned up insignificant correlations. The idea of publishing the correlations was then shelved. "To be honest," said the current state research director, "one of the reasons we didn't publish them was because it wasn't politic to do so." As Kearney recently acknowledged, "When we first started out we naively had greater expectations that we would find the correlates of achievement. We've now come to the conclusion that a state program can't provide that kind of data."

The Department's main problem in 1970, however, was to display its initial results so that they would appear useful without producing another political controversy. Since the Department had committed itself in 1969 to a rough plan for releasing the results, it decided to stick by it and put the best possible face on the disappointing evidence. Among other things, the state agency had promised that "information regarding an individual district will only be released to that district." And in deference to local schoolmen's fears that comparative data on specific districts could make trouble, the Department went on to say, "No 'ranking' of districts on variables will be prepared by the Bureau of Research." Famous last words were these.

The state began issuing reports in the summer of 1970. One showed achievement results and their correlates for Michigan's geographic regions and different types of communities, but not for individual districts. A second report, using summary school district data, presented the relationships between broad school achievement levels for groups of districts (high, medium, low) and broad levels of resources, population characteristics, and so on. Hard to interpret and ambiguous, these reports created very little stir.

Finally, in August, each school district received its own report on student backgrounds, financial resources, and student scores. In addition, norm tables were supplied, allowing each district to compare its mean scores with the state as a whole or with other communities of the same type.

Word of these local reports soon got out. It didn't take long before the Department was besieged with demands to make the test scores public. According to two state agency officials:

> . . . [T]he release of the local reports brought on a torrent of requests by legislators for actual data on "their" districts. The requests were made formally and informally by letter, telephone, and word-of-mouth. . . .

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> [Chairmen of various committees] and several individual legislators all wanted the data. A number of individual members of the State Board of Education felt quite strongly that public disclosure of the data should be made.

Demands for the data also came from the Governor's office and the press. State officials had grossly underestimated public interest in information on specific districts, and curiosity about comparisons.

The Department seemed to be in a pickle. It had made a clear commitment to local schoolmen not to release rankings, but the program had been well-funded ($248,000) by the legislature and supported by the Governor. How could the agency resist their demands to see the data? Delay was the first answer. Board policy on releasing district scores would be thoroughly reviewed in consultation with local school superintendents. But legislators would not hear of it. One irate representative wrote the Department:

> . . . I cannot stress too strongly my shock and amazement at this tactic. Whether or not local school administrators wish to have this information released to legislators is no concern of mine nor should it be of the Department's. As a legislator, it is my position that I have an absolute right to the test data. . . .
> . . . Secrecy and the withholding of information is a classic bureaucratic technique which has no place in a free society. . . .
> Therefore, I expect by return mail the test results of my school districts.

Under continuing pressure, the Department released individual districts' test scores in October.[2]

The repercussions

The results were immediately picked up by the press and widely broadcast across the state. The response was a lively mixture of excitement and horror, and a political free-for-all began. Some groups were delighted. Legislators from the "U.P." (Upper Peninsula) took great glee in berating their Detroit colleagues, whose schools spent more but got worse results. The Michigan Chamber of Commerce fastened upon the data comparing individual districts to show that more money didn't produce better schools. (Later, in 1972, the Chamber buttressed

[2] The Department had actually predetermined the outcome. Earlier in the year, the legislature had been looking for a new way to distribute state compensatory education funds (Section 3), because its old formula was under attack. At the Department's urging, the lawmakers decided to use student background and test data from the new assessment as an index of need. But this required the release of school district test rankings, in order to show the new basis for allocating Section 3 monies. Faced with the inevitable (and blaming Section 3), the Department released the test scores.

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its argument by publishing excerpts from the Department's unpublished correlational analyses showing minimal relationships between achievement and resources.) As one legislative aide put it, "You use these things in a human way. If you can use it to help your purposes, you do it."

Many were plainly disappointed with the results. The state assessment director admitted that "the Governor's office said that the reports are very interesting, but they don't tell me which are the good schools and which are the bad schools." And a legislative aide commented, "The results are so primitive that they are not useful." The assessment provided just enough evidence to stimulate curiosity and conflict, but not enough to answer many questions.

The education establishment, however, bordered on apoplexy. School administrators howled about the "planned superficial and inconsequential" involvement of school people in the program's development, about the inferior technical quality of the tests, about the Department's unethical behavior in releasing the scores, and about the violation of Michigan's cherished tradition of localism. They were, of course, particularly enraged that the press and taxpayers interpreted test scores as measures of school quality, despite their own arguments that achievement depends more on children's home environment and basic intelligence than on school experiences. In fact, in at least two low-scoring districts attempts were made to fire school personnel as a result of the data.

Teachers also were upset. They complained that they had not been involved, that the tests didn't measure what was taught, that they were unfairly being held accountable for student progress, and that the absence of scores on individual students meant that the data were useless to them. Finally, "humanists" challenged the notion that common goals could be established and tested. The real purpose of education, they argued, was not to develop basic competencies of the sort reflected in the assessment, but "to help each learner know himself, respect himself, and become himself as he learns to work with and for others."

Appeasing the critics

The Department was not oblivious to the barrage of criticism. Indeed, a second-year assessment report pointed out, in a typical bureaucratic understatement, that "the 1970-71 assessment plans have benefited from a great deal of involvement of local citizens and educators. Probably no major program operated by this Department has

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received more scrutiny and discussion." This "scrutiny and discussion" made it plain that the program's survival would depend on its ability to respond to its opponents. Since the political climate would not permit a return to the release of test scores only to schoolmen, the agency responded to local pressures in other ways. First, it decided that student background questionnaires would be anonymous (eventually, the collection of all such data was stopped because of political pressure). Then it shifted the program's focus. Rather than collecting data just to aid state decision makers, the program would provide useful data at the local level as well: Individual rather than group scores would be reported.[3] The hope was that this would appease teachers and parents who wanted to know how *each* child performed.

Despite these changes, and increased consultation with local officials, the assessment was negatively received in its second year. In 1971, after the administration of the tests, a group of 43 districts banded together and withheld their answer sheets, pending the satisfaction of certain demands they made on the state. The following year, a task force of the Michigan Association of Professors of Educational Administration released a scathing, if not scholarly, analysis of the assessment. And on top of all this, teachers now complained that even individual scores weren't useful in diagnosing specific children's problems, because the tests did not match Michigan curricula.[4]

More changes were made. The first step was to back away even further in interpreting the 1971-72 results. The third-year report pointed out that the assessment "is not to be viewed as an evaluation of Michigan schools. Instead it is to provide information on school student needs." The following year this warning was expanded, explaining that the assessment program "does not indicate which schools or districts are most effective or efficient." Rather, the report says, "data are intended as an assessment of educational need."

This growing modesty about the usefulness of assessment still did

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[3] This shift was also in response to the Governor's Commission on Educational Reform which, prior to the build-up of local pressure, had called for test results on individual students.

[4] The affair's emotional pitch is exemplified in a 1971 letter to a state official from one prominent school superintendent, a leading critic of the program: "Thank you for your recent invitation to participate in the fourth grade reading panel of the Michigan Item Development Project. As I indicated in a recent telephone call, my personal and professional code of conduct would not permit me to join in a project which I consider both immoral and unprofessional. You see, I'm just not wise enough or egotistical enough to know what every fourth grader in Michigan ought to know or how he should perform. Asking me to participate in this project is equivalent to requesting the Pope to assume the Presidency of an Atheist Society. Be assured that I intend to do everything I can to insure the failure of your project."

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not satisfy school administrators or teachers, and it left taxpayers confused and disillusioned. What had started out as an effort to help reallocate resources by linking them with school outcomes was becoming a "needs assessment," providing data mainly for local use. As such, its relevance to state policy seemed less immediate, thus seeming to undermine the original rationale for the program. By 1972 the assessment had neither met its original purposes, nor satisfied its critics, nor avoided new complaints in the process of trying. If the idea was to succeed, something new was needed.

The new approach

A new program was announced early in 1972. After the assessment in January of 1973, existing tests—which compared schools or districts to state norms—would be replaced by *objective-referenced* tests. These would compare students' performance to specific instructional objectives, rather than to other students' performance. A test that compares one student's reading ability to that of others is norm-referenced. A test that determines whether a student can read a job application or add up the grocery bill is objective-referenced.

Described by the state Department of Education as "revolutionary," this change was based on several key assumptions: First, that agreement could be reached on instructional objectives, and that appropriate test items could be devised; second, that the program could satisfy parents' and teachers' desires to know specifically what students can do, and at the same time provide data for evaluating schools' effectiveness; and, third, that intelligible results could be reported to a wide variety of interested audiences.

Developmental work for the new approach was already well advanced when the announcement was made, for the original 1969 legislation had authorized long-range assessment planning. State agency researchers, influenced by the National Assessment's development of goals and objectives, had begun earlier to develop their own. In early 1970, the State Board of Education had appointed a task force composed of Michigan educators, students, and lay citizens to develop statewide goals. More than a year later, after 25 public meetings, the *Common Goals of Michigan Education* were adopted. Like those adopted in other states at this time, Michigan's goals were little more than a series of platitudes. The real task was to translate them into more specific objectives that could be evaluated and measured.

This process began in the spring of 1971, and it was dominated by professionals. Calling together his curriculum specialists, State Super-

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intendent John W. Porter asked for specific minimum learning expectations for school children. As one participant commented, "The feeling was that as a state agency we ought to be responsible for guaranteeing a basic education for all kids." This was in keeping with Porter's philosophy that if children don't learn to read and write, regardless of their race or economic circumstances, the fault lies with the schools and not the children. Practitioners from across the state in various disciplines (corresponding to the subject matter specialties represented on the state staff) were assembled to develop minimum performance objectives. And later in the year, Grade Level Commissions were established to assist in developing the specific minimums for each level of elementary and secondary instruction. Commission members included teachers, administrators, and curriculum specialists, as well as school board members, parents, and other citizens.

While these developments were under way, state officials also were exploring a variety of new management ideas. Porter wanted to respond to the new conservative climate in the state and to the Governor's criticism of inadequate state education leadership. He hoped that more rational management would help. The latest ideas, drifting into education from industry and the Department of Defense, relied heavily on the pre-specification of objectives. It seemed as though both rational management and the new objective-referenced tests required greater clarity about the goals of education.

By early 1972, the Grade Level Commissions were ready to review tentative minimal instructional objectives. After a protracted search for consensus described by one participant as "probably the most traumatic experience in my educational life," agreement was reached on more than 1,200 objectives in eight basic skill areas. But top priority was given to the objectives in elementary school reading and mathematics, because quick approval was needed to permit the state to replace the old norm-referenced assessment with objective-referenced math and reading tests in hopes of salvaging the fifth year of the assessment. The commissions approved these few minimums in reading and mathematics in June, 1972.

With these in hand, new test development began. After another year for item writing and pre-testing in five school districts, the new objective-referenced tests were ready. They were administered to all fourth and seventh graders in September and October of 1973.

The new tests were narrow in scope. Recall that from all the possible goals of education the Grade Level Commissions had restricted themselves to objectives in certain curriculum areas, and these objectives were to be minimums. From these minimums the state chose

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objectives for only two grades in only two areas—mathematics and reading. And from these, because of time limitations, a smaller set of minimal objectives—"some of the more important" ones identified by the commissions—were chosen for measurement. (All objectives were minimal, but it seems some objectives were more minimal than others.) As a result, the assessment measured only 23 performance objectives in reading and 35 in mathematics at the fourth grade level. The seventh grade instruments measured 23 reading and 45 mathematical performance objectives.[5]

The initial assessment results have now been made available. Each district has received computer print-outs supplying an enormous amount of information. For each student in the fourth and seventh grade, the reports show whether or not the student correctly answered each of the approximately 300 test items, how many items were correctly answered, and whether or not the student mastered each objective. In addition, summaries are available at the classroom, school building, and district level which include the percentage of students answering each test item correctly, and the percentage of students who attained each objective. Finally, a listing of students in each classroom who attained each objective is provided.

So by the close of 1973, the state had established its goals and objectives, had measured a small subset of them, and had distributed some initial results. What has been the program's effect?

The teachers' response

Responses have just begun to reach the Department, and the initial results are mixed. Teachers seem to find the new tests more helpful than the old norm-referenced tests, for they now receive specific information on what each child knows and does not know. But when taxpayers or administrators try to use the results to hold them accountable for student performance, teachers become alarmed. One state teaching specialist, for example, said, "Administrators are out there cutting our throats."

Fearing the worst, the Michigan Education Association (MEA)—the state teachers organization—has become increasingly strident in attacking accountability during the last year. The organization has

[5] For each objective, five multiple choice exercises were included. To avoid success by guessing, it was decided that a student had to answer at least four of the five questions correctly in order for the objective to be attained. Even with the limited number of objectives, this meant 290 exercises for fourth graders and 340 for seventh graders (not counting a vocabulary test and an optional measure of student attitudes also included in the package).

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mounted a determined campaign to derail the assessment,[6] and has enlisted its parent group, the National Education Association (NEA), the country's largest and oldest teacher organization, to use Michigan as a "prototype" for testing methods it will use to fight accountability elsewhere. As part of their joint effort the two groups brought to Michigan in January of 1974 a "blue-ribbon panel of experts . . . to evaluate the educational soundness and utility for Michigan of the Michigan educational accountability model, with particular focus on the assessment component." The panel consisted of Professors Ernest House of the University of Illinois, Wendell Rivers of the University of Missouri, and Daniel Stufflebeam of Western Michigan University.

The panel was chosen after a "nationwide search" for individuals with "competencies that we [MEA and NEA] believe essential to an evaluation" of accountability. Indeed. House has attacked the basic ideas behind accountability, and last summer he helped NEA develop its anti-accountability platform; Rivers has written about the evils of culturally biased tests; and Stufflebeam is an expert on evaluation and a stickler for research design.

We are sure these experts' conclusions were not "dictated" by their employer. But the MEA did not choose other experts with other views. And the report—released in March 1974—was not exactly ecstatic about assessment. The experts raised a variety of technical questions about test construction, validity, and sampling, and they gave the program a few technical pats on the back. But they asserted that the results had been misused and misunderstood, and that most actors in the drama had little or no interest in assessment. They argued that no statewide test could satisfy the needs of all teachers or school districts, and suggested that the only approach to assessment with a chance of being useful would be one with a strong local orientation, created with the participation of all important interested parties. Delighted with the experts' critique, the MEA and NEA are circulating the report across the country.

The initial reaction of parents and taxpayers is less easy to fathom. The idea of specific information on children's skills seemed appealing to them, but in practice it proved confusing. This stems in part from the obscurity of some of the particular objectives. For example, one reads: "Given a statement of equality involving addition, sub-

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[6] The fervor of this effort is best illustrated in a November 1973 cartoon created and circulated by the MEA. It shows Superintendent Porter atop a huge steamroller, with children and teachers lying flattened in his wake. The caption reads: "Steamrolling along with John Porter's education accountability model . . . guaranteed to iron out all problems."

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traction, or multiplication facts and containing a placeholder or letter, the learner will identify the missing number." Another reads: "Given a coordinate system using whole numbers and/or letters and given three pairs of coordinates, the learner will locate the points." While these probably make sense to mathematics teachers, their relevance does not exactly jump out at the average citizen. State agency leaders are aware of this problem, for it is even greater with many of the 1200 or so objectives not yet tested. Porter therefore floated the idea of publishing a check list of simplified objectives that parents could more easily use to gauge teachers' performance. Teachers naturally took a dim view of this proposal, arguing that it represented a gross oversimplification of education and another attempt to make them the scapegoats. The plan has undergone reexamination and is now being pilot tested in several schools.

The information avalanche

Apart from all this, however, the chief problem over the long haul (if there is one) will be managing, understanding, and interpreting the results. Under the new assessment, each district gets a separate score (percentage of students achieving mastery) for each of the 58 fourth grade and 68 seventh grade objectives. By contrast, in previous years each district received only five achievement facts for each of these same grades (a composite achievement score for the district, and the district average on each of four subtests). Instead of 10 outcome facts, each school district now has 126. And the new facts are hardly easier to interpret. The new assessment provides no sense of how many objectives a student really has to master to be "safe" (no one knows).

To further complicate matters, the State Board of Education feels that the old district-by-district comparisons are misleading, because they average out school-to-school differences within districts. (As a scientific matter, there is much to be said for this view.) The Board has thus expressed interest in publishing results not simply for the state's 600 or so school districts but for the 3,000 schools within these districts. If this plan is implemented, though, the number of achievement facts in the state report would increase fivefold to 378,000, more than 60 times as many as appeared in compilations under the old assessment system. A Department official estimated that such a report would be about 2,000 pages long, and the printing cost for 8,000 copies would be $96,000—almost half the price of the entire first-year assessment. Despite the scientific merit of publishing results for

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individual schools, the idea is unworkable for practical and political reasons.

This is aptly illustrated by reactions to the state's earlier and much more modest reports. Senator Gilbert E. Bursley, Chairman of the Senate Education Committee, refers to them as "Sears Roebuck catalogues." One can just imagine his colorful language if the Board follows through on its more expansive plans. One cannot, however, picture Bursley and his colleagues (not to mention local school board members or taxpayers) successfully making sense out of a report containing more than a third of a million achievement facts. Moreover, the difficulty could increase, because the state's assessment plans are far from fully realized. For one thing, the agency intends to move beyond testing basic skills at the fourth and seventh grades, and to test students at the first and tenth grades, as well as at the end of high school. It also wants to move beyond the few dozen objectives used so far to the 1,200 devised by its team of goal specifiers and test item writers.

The consequences boggle the mind. For example, a trial run of the first grade assessment (which had to be individually administered with these young children) wound up taking 30 hours per pupil just to test 50 minimum performance objectives. If a teacher started testing the first day of school, and tested all day, every day, until all 30 students had been individually assessed (assuming five hours of testing per day and an hour off for lunch in a nine-to-three school day), she would complete testing at 3:00 P.M. on the last day of the school year. (In its first year, the old assessment took only two hours.)

This is ridiculous, of course, and the Department's testers realize it, but it does demonstrate some of the costs of being increasingly specific about objectives. In this case, the agency will probably either have to give up reporting all individual scores (by testing only samples on some objectives, which will make the results less useful to teachers), or it will have to test only high-priority minimum objectives (as it did this year, which raises questions about what minimum objectives really are). The idea of providing decision makers with better information can very quickly produce overload.

This problem is evident in the reaction of school administrators. Publicly, they say they are happy that published comparative rankings have been abandoned—after all, this was largely their victory. But they also seem generally confused about the meaning of the new tests. According to a Michigan testing specialist, one district was very concerned about its results, "but didn't know whether to feel happy or not." To add irony to the confusion, some schoolmen are

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now secretly visiting the Department, hat in hand, asking for some kind of composite scores so they can understand the results and compare their progress.

Early in March 1974, the state education agency tried to deal with the comparison problem by sending each district a report about statewide performance on the new tests. In a covering memo, the state assessment supervisor recognized that "comparing your local results with statewide data is, of course, a normative comparison but one which will be of interest to local educators." But such comparisons were not exactly easy to make. Instead of a composite score, each district got a list of every tested objective and the percentage of students in the state who had achieved it. This meant every district that wanted to figure out where it stood in relation to the state as a whole had to compare its students' performance on 126 separate objectives (each one of which had five test items) with statewide performance on the same 126 objectives. The state offered no suggestions about how these comparisons might be simplified.

The meaning of minimums

The reason for this information overload might seem to be simply that no one had bothered to reduce the masses of new evidence into more manageable form. But that isn't a reason—it is only a symptom. The real problem is figuring out exactly what a minimum is. If the state were sure what every student should learn, when he should learn it, how it should be measured, and what the relative importance of various objectives were, then scores on the resulting list of objectives could simply all be added together, and each district could have a simple summary index of minimum attainment for all its students or for each grade, or school, or classroom. But the state is not sure about any of these points. The supervisor of assessment noted in the memo referred to above that the tests were based on several "assumptions": that the objectives described essential skills; that those skills could and should be achieved by all students; and that the tests correctly measured the objectives. But he then went on to list a page of reasons why, in many cases, these assumptions might be incorrect, and closed by noting that while the test represented "the highest level of quality permitted by the present state of the art . . . the test data—just as any test data—are subject to error."

The state had good reasons for being diffident on this matter, for the attempt to define minimum objectives raises a number of serious conceptual problems. The first of these centers on the question:

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Minimums for what? A state assessment report describes minimums as "the set of objectives considered necessary for continued success in schools." This implies those skills necessary to *make it in the educational system* (e.g., ability to read novels). Superintendent Porter, on the other hand, has written about the importance of acquiring "the minimum school skills necessary to take full advantage of the adult choices that follow successful completion of public education." This seems to imply those basic competencies needed to *make it in society* (e.g., ability to read job descriptions). There are other views as well, but the point is that these different concepts of minimums rest on different views of the purposes of schooling, and thus have implications for what the schools should teach. The first concept might suggest a focus on great literature; the second on basic reading and vocabulary; a third, perhaps, on developing a sense of self-worth. The last 10 years of research on the schooling process reveal little scientific (or social) consensus on which view is right.

Yet even if agreement were reached on the schools' goals (say, to develop basic competencies to make it in society), it doesn't necessarily follow that there will be agreement on the most important minimum skills. There is disagreement among math specialists, for example, about whether problem-solving skills are more important than computation. Other experts argue about the definition of reading. The terms "minimum" and "basic skill" have great political and educational appeal, but they cover a squirming mass of political and pedagogical differences.

A second major problem in defining minimums is the appropriate level of difficulty. This dilemma is well illustrated by Frank Womer (the former research director for the National Assessment, now at the University of Michigan) in a 1974 paper with the uncharacteristically harsh title, "Danger: Assessment is Abroad in the Land." Womer, a supporter of Michigan assessment, shows that almost all of the 24 Michigan school districts he examined did poorly on many minimal performance objectives. This could be interpreted, he believes, "to mean that not a single district in the state is meeting minimum objectives." But Womer thinks this conclusion is "unwarranted." Either the minimum objectives are not really minimums at all, or the test items were badly written and turned out to be too hard. Womer isn't sure which, and he thinks the results are puzzling.

Department officials, on the other hand, believe Womer has misinterpreted the true meaning of a minimum. To them, the term represents what *the professional thinks all students should master* (this is, of course, simply a new norm). Since everyone knows that

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many children are unable to read and write, their argument goes, many children obviously fail to meet the minimums. And presumably, as students show greater success in achieving old minimums, new ones can be established. The critical point to notice here is that the old norm—which was based simply on average student achievement—has been replaced with a new norm based on professional judgment.

The politics of minimums

If the minimum objectives were simply a source of private delectation for state officials, this would be little problem. But the minimums are public. They are being used to hold teachers and administrators accountable, and if they stick, they will shape curriculum as well. Under these circumstances, people will inevitably fight over them. Teachers will try to make the minimums really minimal—or to throw out the whole program. The state will resist making them too easy, lest it lose a lever for forcing change. And these conflicting views, of course, will be reconciled not by scientific advances but by political bargaining.

It isn't clear how things will turn out. If the minimums are made really easy, the assessment results would not be very useful, since the data would simply confirm that most children can accomplish the bare bones. On the other hand, the state's version of minimums might prevail, and the schools might really focus their efforts on those who have not achieved them (as the state leadership hopes). In this event, the outcome would be hard to predict. If the schools were to succeed, it would take a long time and much more patience than anyone has displayed thus far. But success might not come anyway—either because teaching was poor or because more effective methods could not be found. Then, too, the state's hoped-for focus on poor performers might backfire. Parents might favor in principle *all* children achieving certain minimums, but not be willing in practice to pay the price. MEA spokesmen are already suggesting that teaching to minimum objectives will have good students sitting in classroom corners twiddling their thumbs, while teachers work with the worst pupils. Whether this would be a good thing is, of course, a question of value priorities. The idea of children in trouble getting more help appeals to us, but our children aren't in Michigan schools, and it isn't hard to imagine parents moved by the contrary concerns. If teaching to minimum objectives does reallocate teacher time to the poorest students, there could well be a political response by

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parents designed to insure that all students receive their "fair share" of schooling.

All of this suggests that the new assessment program has replaced a familiar can of worms with a new one. The old norm-referenced tests reduced all the information to a few statistics, which were often misleading. The new objective-referenced tests produce a veritable avalanche of information—but as yet there is no obvious way to interpret it or to use it to frame intelligent decisions. The old tests sidestepped conflict over appropriate minimums by comparing students against one another. The new tests instead spotlight those differences by comparing students against specific instructional objectives. Thus they raise difficult questions about what the right minimum objectives ought to be, at what level they ought to be set, who should set them, how they ought to be used, how extensively they should be measured, and how they should affect classroom life. This is not to say that the new approach is foolish, or that some of these problems can't be resolved by time and science. But the fact is that the Department is currently moving full steam ahead with a program that is extremely complex, not yet scientifically mature, and thoroughly political. One of the originators of Michigan's assessment, reflecting on the complexities of the program he started, said recently with a smile, "Sometimes I wake up and ask what the hell I've got myself into."

Assessing assessment

It is, of course, too early for any definitive judgments about assessment in Michigan. But the last four or five years' experience does offer some evidence on this innovation's effects.

One hope among state officials was that the collection and publication of data on school resources and outcomes would provide a scientific basis for allocating more resources to poor districts. That notion has more or less fallen of its own weight, for the assessment—like most other large-scale research on school effects—turned up little relationship between school resources and student performance. If anything, the assessment provided comfort and ammunition for the enemies of liberal reform.

Another hope was that the assessment program could provide an evaluation of the schools that distinguished effective from ineffective programs. This way taxpayers could see whether they were getting their money's worth. That hope also has been dashed. The data were not really appropriate—though they were used anyway—and the

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state did its best to avoid this problem by disavowing any evaluative purposes.

Another expectation was that better information would make decisions better by making them more rational. Michigan educators don't seem to have any more idea of what a "better" decision is than we do, but it would be hard to find anyone in Michigan who thought that the assessment had made things more rational. The experiment has generated a steady background of political conflict and confusion, intermittently punctuated by major explosions. One consequence has been to force several major revisions in the assessment program. The most important of these, the switch to objective-referenced tests, has posed an entirely new set of problems and generated a mountain of novel information whose meaning and relevance to policy is unclear. At this point, one could make a reasonable case for the proposition that new scientific information is more likely to unsettle things than to provide the basis for greater political rationality.

One source of these difficulties, of course, was the power of local school districts and statewide professional organizations in Michigan school politics. It was to be expected that the assessment would run afoul of these established interests. But perhaps the central problem was the enormous political contradiction that the state educational leadership tried to straddle. It wanted to develop a new assessment program that would help professionals diagnose and solve students' learning problems, but it also tried to present this diagnostic system as a way of evaluating the performance of school professionals. It is not intuitively obvious that diagnosing students and assessing teachers are the same thing, but to attract political support for the assessment, the state rolled these two aims into one ball and sent it spinning toward the schools.

The result, however, was that the state had little room to figure out whether the ideas underlying the assessment actually worked. More careful development efforts might have helped determine whether the educational minimums and tests could have been further refined. They could have shed some light on how the evidence might actually be used. They could have provided time to determine what relationship, if any, there is between what the objectives specify, what the tests measure, and what skills adults use. They might even have provided an opportunity to figure out the relationship between the tests and objectives on the one hand and what the curriculum contains and what teachers do on the other.

Lacking the chance to answer these questions, the state agency

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has been forced to say (and think) contradictory things. It tried to sell the program to teachers as Michigan's newest "teaching tool," because if teachers can't (or don't) use it, no one is going to be improved, even if they are assessed to death. At the same time, the agency told taxpayers and legislators (and itself) that the assessment could help measure "the productivity and effectiveness of administrators and teachers." Understandably, this gave administrators and teachers acute indigestion, and thereby alienated the very people upon whom the state depends for political support in continuing the assessment and for active cooperation in using the results and improving the schools. In trying to make the program serve varied interests, the state wound up fanning the flames of conflict.

That, of course, is what some proponents of assessment had in mind. They hoped that by making information about schooling public, what seemed to them a closed, cosy, and professionally controlled system of decision making in education would be opened up to new forces, new ideas, and reform. It certainly is true that in Michigan there have been new ideas, more debate, and sharper conflict. It also is true, as some advocates of assessment had hoped, that the arguments about schooling have changed from focussing on resources to focussing on results. These changes alone, they maintain, are a great improvement. And if the new frame of mind persists, they think ways will be found to improve the schools' performance—even if the current program flops.

Reformers habitually make this argument, and we are not inclined to quarrel with it in the abstract—for every case in which it seems incorrect, there is another in which it seems quite plausible. But in this particular instance we are dubious because Michigan's assessment program has created so much confusion, the stakes are so high, and there is so little evidence that the program has improved decision making or shifted power.

II. Incentives for performance

One could argue that what was missing was some independent source of leverage for the state education agency. It might have been in a better position if it could reward those districts and teachers who succeeded, and punish those who didn't. This might have given the state some power, rather than leaving it politically dependent on the very people and agencies it was trying to whip into line. As it turned out, such an opportunity materialized soon after the assessment began.

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Although the reasons were partly fortuitous, the Michigan legislature in 1971 passed a law that conditioned local receipt of state compensatory education funds on local schools' ability to raise student test scores. The idea was simple: Give state money to districts with the most underachieving children, but give it on the condition that they agree to improve children's performance. If districts meet their part of the bargain, they will get their full quota of money. If they don't, they won't. In addition to giving them greater power over schools and teachers, the notion had the great virtue, for the reformers in charge of the state education agency, of holding schools responsible for children's failure, rather than blaming the children themselves.

This new program (Chapter 3) was enacted in October of 1971, replacing a more modest compensatory education effort. The new version was designed to last three years. During the first year (1971-72) funds were to be distributed to the neediest districts, using the 1971 assessment test results as an index of need.[7] The districts with larger percentages of students scoring below the 16th percentile would be funded first ($200 per elementary pupil), until the available funds ($22.5 million) were exhausted. Once the money reached the districts, the lowest-achieving children were to be identified on the basis of local test scores and, in some cases, teacher judgment. Those selected were to remain in the program for three years, or until they left school or graduated from the sixth grade.

Each district was required to spell out specific objectives and an evaluation design. Beyond that, the money could be spent virtually without restriction. The hooker, however, lay in the incentive provision, which was supposed to go into effect after the first year. Second-year funding (1972-73) would depend on first-year gains on locally administered pre and post tests, and third-year funding would ride on second-year gains. Except in a few instances, the chosen objective was one month's academic growth for each month in the program, as measured by locally chosen standardized tests. The stipulation, however, was that for each pupil achieving 75 per cent of this objective the school district would receive a full per pupil allocation for the *next* year. For each pupil achieving less than 75 per cent, the district was to receive a lesser amount, proportionate to the gains achieved.

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[7] This, by the way, provided a useful rationale for continuing the assessment program—it could be used directly as the basis for allocating funds. In fact, state officials now point to this as a major program objective, even though it was never intended when state officials began to develop the assessment program.

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In effect, then, the state told local districts that they had to perform in order to get Chapter 3 money. But it also told them that acceptable performance was substantially less than the actual instructional goal the districts were encouraged to set. It is not entirely clear how much academic retardation the 75-per-cent standard would allow during a student's three-year career in the program, but it would not be trivial. State officials argue, though, that even this would represent a considerable improvement over previous performance for such students.

But if there was some uncertainty about the nature of the performance criterion, this problem—like many others—never surfaced during the legislation's consideration and passage. This was because the testing and incentive provisions were relatively small items, buried in the recesses of a large and complicated annual appropriation measure. They attracted little attention as the 1971 act wound its way through the Michigan legislative maze. "The incentive provision was introduced and passed before we knew what had happened," one administrator noted.

The phantom incentive

Shortly after the law was passed, the details of Chapter 3 started filtering down to the local level. According to one observer, it "raised all kinds of hell." Schoolmen were particularly upset about the accountability provision. They saw it not as an incentive for action but as a penalty, pure and simple. "Remember, you don't get any reward if kids do better than the objective," said one participant. In the minds of many local educators, the incentive provision was clear evidence that the state leadership was out to blame teachers for student failures.

Aside from the fury over accountability, uncertainty about Chapter 3's operation created a good deal of confusion. The program was not passed until after the 1971 school year had started, many local Chapter 3 programs didn't get under way until January of 1972, and it was a full year and a half before the state promulgated official program regulations. As a result, the state agency was hit with a barrage of objections that implementing the accountability provision in the first year (1972-73) would be unfair. Apart from the late start and general uncertainty, it was argued that if less money flowed the next year, newly hired staff would have to be fired and children would receive fewer services.

Faced with a chorus of complaints, Porter recommended that the

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legislature write a first-year waiver into the law. Chapter 3 was thus amended in the summer of 1972, allowing second-year monies to flow in 1972-73. The law continued to stipulate, however, that the second-year performance results would determine the allocation for the third year of the program (1973-1974).

In the summer of 1973, Porter and the legislature once again had to face the accountability question. Test results for 1972-73 indicated that school districts, particularly in Detroit, would lose several millions of dollars if the incentive provision stuck. In fact, more than one third of Chapter 3 students had fallen short of the 75-per-cent goal, causing a possible loss of $5 million. Once more there was intensive local lobbying to waive the incentive, and once more Porter and the legislature yielded to the pressure: Districts would be given a "second chance" at the money. A new provision was written so that school districts would "provide a different delivery system" in 1973-74 for those students "who did not achieve 75 per cent of prescribed minimum performance objectives in 1972-73." If districts implement a new "delivery system" and if the students reach 75 per cent of the so-called performance objectives, then the school district will, after all, get the full amount. One wonders whether, if this doesn't work, the legislature will write another waiver into the law later this year, amid some rhetorical celebration of accountability. "This is an election year," said a legislative aid. "It is no time to be taking money from anyone."

This suggests that Chapter 3's highly touted incentive has proved to be more a myth than a reality. Several political professionals explained why:

> That was a good bluff, because when Detroit didn't measure up, what do you do? Here's a district that's about ready to go down the tubes. So what do you do? You give them the money.

> The excitement is around the performance standards. The legislature every year, when faced with that dilemma, always writes in a forgiveness clause. . . . When it means a loss of four or five million dollars to Detroit the legislature will get on the stick and the Department won't stand in the way.

The legislature might have the stomach to withhold money from small school districts, but one half of the Chapter 3 funds go to poorly scoring but well represented Detroit. In the present climate, with the present distribution of Chapter 3 funds, Michigan's payment-for performance idea simply is not politically viable.

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A rhetorical triumph

The story should end here. The incentive has not been implemented. No one has been rewarded for success, or punished for failure. And everyone who notices such things in Michigan education knows it. Chapter 3 has not been implemented because local districts would not stand for it. As is often the case with a reform, though, reality does not intrude upon the claims of success made by its advocates. During the last year or so the program has been given national attention as an important and successful innovation. Superintendent Porter claims that he has "hard data" to show that the program is making a difference. In 1973 testimony before the U.S. House of Representatives General Subcommittee on Education, Porter said:

> Evaluations for 1971-72 of our state compensatory education program are very encouraging. All over our state we have information to indicate that indeed we can make a difference in the performance of students in the basic skills. . . . In most of our school districts students are for the first time beginning to improve their performance. . . .
> . . . The critics are saying we don't make a difference. We couldn't get the hard data on Title I. . . . We had to have the hard data to say to the critics: Yes, public education does make a difference. And I suspect you are going to see more state legislatures wanting to move in this direction.

When asked why Chapter 3 apparently was performing better than Title I, Porter pointed to Chapter 3's unique provisions, particularly the pre-specification of objectives and the institution of monetary incentives.

Last year Porter testified twice before the Congress, specifically on Chapter 3. The program has been written up in the *New York Times, Compact,* and *U.S. News and World Report.* And Michigan's activities are receiving widespread attention in other states.

All this is quite remarkable, in view of the fact that the Chapter 3 incentive has been, as they say, inoperative. One is at a loss to explain how this fact has managed to escape Congressional staffers and reporters. It is surely a testament to the state Education Department's rhetorical abilities that it has been able to make a smashing national success out of a program whose vitals have been excised by the legislature two years running.

In case this fact had escaped local educators, we decided to conduct a modest exploration of Chapter 3's impact on schools. Two things stand out. First, the activities funded under Chapter 3 seem to be little different from those funded under other compensatory programs in Michigan, particularly ESEA Title I. Funds have been used mainly to hire additional staff, ranging from remedial reading special-

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ists to teacher aides. Second, students in the same classrooms often receive supplemental assistance from several sources. It is not uncommon to find some students who are Chapter 3 pupils, others who are Title I students, others who are both, and still others who are neither. And all of them may be receiving the same type of educational assistance from a specialist funded by Chapter 3, or Title I, or both. In many cases, neither the specialist nor the classroom teacher will be able to identify the Chapter 3 or the Title I students. If these observations are accurate (the state's Chapter 3 coordinator agreed with them), they suggest that the Chapter 3 incentive (or the threat of losing money) doesn't seem to have much distinct impact at the classroom level.

When visiting schools, we also asked about the implementation of the new amendment (Section 39A) requiring revised "delivery systems." According to state guidelines, schools were to have descriptions of the changed delivery systems on file. We found some documentation in two of the three school districts we visited, but the descriptions were not very informative. One description read: "Changed grouping practices. Lowering class sizes. Providing reading-readiness." Another called for a "program to strengthen discovered areas of need, and prescribe treatment." A third prescribed "individualized instruction and remediation in reading and math."

When pressed for more specifics, principals were not much clearer. One said that the reading program for the entire school had been changed, but that nothing special was happening for the Section 39A students except "more monitoring." Another said the program is "probably a little bit more of the same." One program coordinator, however, was more enthusiastic about the concept; certainly, it does provide an aggressive administrator with a lever that could be used on teachers. But in the five schools we visited it meant nothing—except, perhaps, try something slightly different, or try harder.

Alternative approaches

All of this is not to say that Chapter 3 is a waste of money, or that principals and teachers aren't enthusiastic about extra aid. Nor is it to say that changing delivery systems is a bad idea. Certainly, the underlying philosophy—change the program, not forget the child—is appealing. But there is little reason to think that the on-again, off-again accountability provisions of Chapter 3 have been a clear and distinct force in Michigan schools. The Department may claim the program is a success, but this is based on only one year of test results

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(the first year, at that), under the most ambiguous conditions. There are a dozen reasons to be dubious about such episodic results. But the chief point is that this is the wrong issue. The real question is not how the program has affected students, but why it never got into gear.

The answer is that incentives for performance were politically unacceptable. The state agency's fiscal penalties and rewards were no match for the political power of local school districts and education professionals. As with many reforms, then, one doesn't know whether this version of accountability would work educationally (i.e., whether schools would respond to fiscal incentives by improving education), because it has not worked politically. This may not be the fault of the idea itself—economic incentives for performance are not exactly an unpopular notion in the United States—but knowledge about its workability in education has not been much enhanced by the way it was managed in Michigan. Chapter 3 was rushed into operation with minimal preparation and no systematic analysis of alternative approaches. Like most innovations, Chapter 3 was put together and worked out on the hoof. As often as not, this seems to create more problems than it solves.

We are not convinced that economic incentives are socially desirable in education. But if they are to be adopted, it seems reasonable to us to explore other approaches to setting performance standards for schools. School districts, for example, could be offered rewards for improving performance in ways specified by the state, but initial participation could be at the discretion of individual districts. Or performance agreements could be arranged between individual schools and the state, making local district-wide decisions (which are often nastier) unnecessary. One wouldn't expect a mad rush to embrace this idea, but some districts or schools would probably try. Another possibility would be for the state agency to contract with local teachers, parents, or administrators who wanted to start new public schools (or take over existing ones). This would be a more radical step, and surely less easy to achieve; but there is evidence (from the Harlem Street Academies, for example) that there are parents, students, and teachers ready to try and able to perform. Or the state could offer districts grants for planning and developing a performance system over the course of an initial year or two, on the understanding that only promising districts would be selected for an operational test. Again, this would hardly guarantee success, but it would remove some of the political problems of starting with a full statewide operating program, and would allow more elbow room to develop a new approach to schooling.

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We have no particular brief for any one of these ideas, though each is appealing in one way or another. None of them seems less appealing in principle than the approach Michigan adopted. Their exploration would seem sensible, even though the Chapter 3 experience does suggest that economic rationality is not about to take education by storm.

III. Conclusion

Accountability through science has not been a smash hit in Michigan. In fact, things have turned out in a rather contrary fashion. Instead of showing the need for resource allocation to the poor, assessment seemed to support the opposite idea. Instead of clarifying educational issues, assessment obscured them in clouds of new and problematic test results. Instead of improving achievement by penalizing schools for non-performance, Chapter 3 raised a storm which resulted in the program's emasculation.

Most observers and participants in Michigan would probably agree that things are now more unsettled, but some would argue that this is all for the good. Conflict creates attention, state agency officials contend, and attention is required for action. The problem with this view, however, is that successful action also depends on sustained political power—something neither the ambiguous assessment program nor Chapter 3 has really provided. It may be that with time, research, and modest field trials, things can be improved. Certainly an effort ought to be made. But if Michigan is any guide, at this point scientific accountability hardly merits full-scale implementation.

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The Safety Regulators

Today the debate over public regulation of business practices has largely shifted from the ideological to the practical level. We no longer are likely to hear regulation damned because it is "un-American" or praised because the Swedes or English do it; rather, the argument is about the proper definition of cases in which the protection of third-party interests requires public action, and about the consequences of such action as has been taken. In scope and ambition, if not in efficacy, American regulatory practices are currently in the vanguard—many European nations now copy methods invented here.

Public criticism of public regulation in this country has been of two sorts: charges of favoritism, incompetence, and even corruption levied by the followers of Ralph Nader, and complaints of economic inefficiency made by academic economists. Both kinds of criticisms deserve serious consideration, though the former is often marred by its moralistic and prosecutorial tone, and the latter is frequently made inaccessible by the highly technical nature of the analysis. Their collective impact, however, has been to suggest that regulatory officials are either venal or stupid, and perhaps both.

In the belief that matters were more complicated than this, The Public Interest *commissioned a series of three studies of regulatory efforts in the field of safety. With the support of funds from the General Service Foundation, research was conducted in three separate agencies in an effort to discover how a regulatory strategy is chosen and what happens when it is applied. In the article that follows, Steven Kelman describes the effort of the Consumer Product Safety Commission to develop a "rational" strategy—one based on a systematic gathering of evidence and an explicit set of criteria for choosing a course of action. Articles in subsequent issues by Marc Landy and Paul Quirk will focus upon the Occupational Safety and Health Administration and the Food and Drug Administration, and the somewhat different kinds of problems these agencies have confronted in attempting to carry out their regulatory mandates.*

—James Q. Wilson

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Regulation by the numbers —a report on the Consumer Product Safety Commission

STEVEN KELMAN

In 1972 Congress created a new federal regulatory agency, the Consumer Product Safety Commission (CPSC), and empowered it to deal with the safety of virtually every consumer product in use. Previously, household products subject to safety regulation had been the exception rather than the rule. Specific laws—the first dating from 1953 but most having been enacted during the consumerist boom of the late 1960's—had given various federal agencies regulatory power over flammable fabrics, refrigerator doors, household poisons, and toys. The new law, the Consumer Product Safety Act, applies to every type of consumer product not covered by other legislation—which is to say, everything except automobiles, cosmetics, drugs, food, and cigarettes.

By giving the new commission such wide powers, Congress inevitably also gave it extremely wide discretion. The enabling act mandates the commission "to reduce unreasonable risk of injury from the use of household products," but leaves it to the commission to decide what "unreasonable risk" is and how to reduce it. In fact, the CPSC can do any of a number of things about any of the thousands of consumer products on the market. It can arrange educational campaigns on radio or television (paid for by government) to encourage safe

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use of a product. It can ban products. It can issue design standards that products must meet. It can go after individual products, one by one, on the basis of consumer complaints or news stories, or it can pursue broad categories of products on the basis of its own independent determination of their relative riskiness. And its decisions in these respects are consequential in the extreme, affecting the lives of consumers and the well-being of manufacturers and retailers. The question such an agency faces is: How does it decide what to do?

The case of the baby crib standard

One of the first actions taken by the newly-created CPSC was to promulgate mandatory standards for the maximum allowable distances between slats on all new infant cribs sold in the United States. The most common sort of injury involving small children and cribs is the obvious one—a child climbs up the sides and falls out. But about 150 fatal injuries were occurring annually in a completely different way. Babies, invariably less than a year old, dangle their feet between two slats. Their feet dangle down far enough so that the child's own weight causes the rest of the body to start falling through the slats toward the floor. Then, if the child's buttocks are not wider than the distance between the slats, the child falls all the way down until stopped by its skull—and strangles itself.

This problem first came to light in 1968, when staff members of the President's Commission on Product Safety, the study group which recommended the establishment of the CPSC, were looking for dramatic problems to gain publicity for the commission's hearings on the need for a federal regulatory presence in the product safety field. Through the Dade County Medical Society they learned of a Florida man who had been waging a campaign against crib manufacturers ever since his child had strangled itself between the slats of its crib. In the commission's public hearings, it developed that neither government nor the crib industry had known about this hazard—nobody was analyzing injury and death statistics in a way that would have revealed its existence—and that nobody knew what a safe distance between crib slats would be.

After the hearings, the commission made an informal request to the crib industry—which, in response to the hearings, had formed a trade association and a safety committee—to come up with a voluntary standard for slat spacing. The industry was told that its traditional slat distance of about 3.5 inches was "too high," but beyond that no advice was offered. The trade association consulted a chil-

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dren's hospital in Massachusetts, which said that, in the absence of technical knowledge on the subject, they thought a distance of 3.25 inches would be "safer." The trade association proceeded to suggest this figure as a draft voluntary standard.

Meanwhile, the study commission ended its work and a number of staff members joined the Food and Drug Administration's Bureau of Product Safety (BPS). (All BPS activities are now performed by the CPSC.) The BPS had contracted with researchers at the University of Michigan to conduct a major study of children's measurements to determine what size holes children of different ages can get their fingers stuck in, how far children can reach across a kitchen range surface to turn on gas switches, and the like. When informed by its new staffers about the crib slat problem, BPS asked the Michigan researchers to make a special study of infant buttocks measurements. Their report was published in December 1972 under the title, "Selected Infant Anthropometry—Crib Slat Sub-Study." Using this document, the BPS staff concluded that a slat distance which would protect 95 per cent of all infants was 2.375 inches, and that the industry's voluntary standard of 3.25 inches was inadequate. In April 1973 a notice was published in the *Federal Register* mandating a maximum slat distance of 2.375 inches for all new cribs sold in America.

The 2.375-inch figure, although it surprised the industry, was the very opposite of a number arbitrarily pulled out of a hat. In fact, the justifying paper published by the agency, entitled "Determination of Inter-Slat Spacing Requirements," reads, depending on one's prejudices, either as a model or as a parody of bureaucratic precision:

> It was found that buttocks depth (measured from front to rear) was the most critical measurement as related to penetration of the crib inter-slat spaces. This measurement, buttocks A-P (anterior-posterior), along with ten other measurements were taken at 0.5 PSI (pounds per square inch) contact pressure. Tabulations for all measures were provided along with tolerance limits for the 5th and 95th percentiles with 95 per cent confidence.
>
> These data were not calculated for a compressed condition nor with the addition of diapers. The effect of diapers was subsequently tested on two infants using cloth (Curity Stretch) and plastic disposable (Pampers) and found to add only 0.1 cm to a maximum of 0.3 cm. On the other hand, the effect of tissue compression appeared to be very important, ranging from 0.5 cm at 0.5 PSI pressure to 1.6 cm at 1.5 PSI pressure for the A-P buttocks.
>
> From the table on page 26 of the sub-study and the data on page 19, we calculate the required spacing as follows:

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	7.4 cm		A-P buttocks measurement
+	.3 cm		Diaper effect
	7.7 cm		
−	1.6 cm		Tissue compression effect
	6.1 cm		
	6.1 cm	=	2.4 inches
Selected			2.375 inches

The CPSC's crib slat safety standard represents something like an ideal case of product safety regulation, displaying all the potential advantages of such regulation and none of the pitfalls. The decision to intervene in the marketplace with mandatory rules involved a hazard of which most consumers are totally ignorant, and the risks of which are assumed involuntarily by an infant unable to protect itself. The cost of making the product safe was minimal. The safety change did not reduce the utility or attractiveness of the product, and the likelihood that the standard would indeed reduce injury and death was extremely high. The CPSC's decision making process followed textbook rules for good regulatory behavior. The standard was based on a sound technical study. The regulators had their own sources of expertise, which in this instance were far superior to those of the industry being regulated. As the crib expert in the agency put it, "The industry concedes that I know more about crib safety than they do." And the standard was immediately accepted as appropriate by all concerned. Unfortunately, things do not usually work out so neatly in safety regulation.

The price of safety

In principle, to be sure, safety is a non-controversial goal. But it is not the only, or even a major, consideration when people buy products. If safety costs something, in terms of money or product utility, that cost must be weighted against other goals. It is not hard to make household ladders less likely to tip over—but it costs money. We could reduce the risk of injury from kitchen knives by making them less sharp—but that would reduce their convenience and usefulness. The question is always whether the safety improvement is worth its cost in money or convenience.

Whenever safety is debated on the floor of Congress, some legislator inevitably rises to object to the very idea of putting a price on safety, for which, it is asserted, "no cost is too high." But this is silly, as we are constantly and necessarily making decisions that implicitly

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balance risk against cost. Traffic authorities decide whether to place stop signs, lights, or traffic policemen at intersections. Beach officials decide how many lifeguards to post. Hospital administrators determine how big a supply of plasma to keep on hand. Safety regulators are in a similar position. They too strike balances between safety and other goals, and this in turn makes safety regulations controversial from the point of view of the consumer, who pays for them, and the producer, whose profits, market shares, and jobs can be drastically affected by them.

Some free market advocates would argue that, since markets are the most efficient means of striking balances between the price and other qualities of products, the government should not regulate product safety at all. If any significant group of consumers desires a certain safety feature, they argue, some producer will come forward to satisfy this demand by manufacturing the product with that feature. If the safety feature does not appear on the market, it is because few consumers want it enough to pay for it; under these circumstances the government, they hold, has no right to step in and force the safety feature on people—i.e., make them buy more safety than they want.

One strength of the market in its current form is that it does allow consumers to choose how much they are willing to pay for increased levels of safety. One can in fact purchase different types of household ladders having different degrees of stability; the safer ladders cost more. Safety glass, also more expensive, is widely marketed for those who wish to construct glass doors with it. The market allows individuals to choose products with different degrees of safety for different types of uses. A "do-it-yourselfer" who uses home power tools once a year might not want or need the same level of protection as one who uses power tools every weekend. By contrast, a mandatory standard for the flammability of mattresses penalizes through increased cost those who do not smoke in bed as well as protects those who do.

One problem with the extreme anti-interventionist position is that the market is subject to serious imperfections. For the consumer, the most obvious of these is that many of the risks associated with products are hidden, so that he assumes them involuntarily. How can a consumer judge for himself whether a television set is likely to explode, a power tool to electrocute him, or a glass bottle containing soda to blow up in his face? Another problem is that of small children, who are largely powerless to shield themselves against a range of hazards, from sharp pins used to fasten the eyes of a doll to burning pajamas.

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There is a conceptual difference between a consumer's choosing among competing products on the basis of their relative utilities, and choosing on the basis of their relative risks. If I purchase a certain brand of orange soda, I consume it and am in an immediate position to compare its taste with that of competing brands. But one cannot usually compare the riskiness of two products in the same way. Product injury is a statistically uncommon event. People are not always injured when they purchase a brand that is riskier than its competitor, and therefore they have less evidence by which to make comparative judgments. Furthermore, it is often unreasonable to expect consumers to know what it is about a product that is dangerous, and thus to be able to demand appropriate safety features. It required a morphological study of children's weights, applied to an engineering study calculating the center of gravity of children sitting on tricycles, to determine what design characteristics make tricycles unstable when turned around a corner. A parent could hardly judge the safety of competing tricycle designs even if he wanted to. Similarly, it is unlikely that many consumers know that about half of all power mower injuries occur when a stone caught in the high-speed rotary blade is expelled at high velocity back toward the user. Yet such knowledge would be important in making a decision as to whether to buy a power mower with a back guard against projectiles.

For the producer, trying to compete on the basis of safety presents a number of difficulties. In cases such as those above, the customer is not likely to be aware of differences in safety between products, but only of the additional cost that these safety features impose. Advertising, which might inform consumers about safety features, must usually be short and snappy, but explanations of safety features are often long and technical. Furthermore, the public may regard safety claims as self-serving and unreliable. At the retail level, it is probably impossible to force salesmen to tell people about safety features—and this problem obviously becomes even more severe where products are sold in self-service stores.

Social costs and benefits

A second argument for product safety regulation is based on the comparative social costs of safety and accidents. If safety has a cost, so do injuries—in medical treatment, lost working time, and so on—and these are by no means always borne solely by the individual consumer. Where the total costs of increasing a product's safety are smaller than the extra costs entailed by leaving the product as it is,

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there is a public interest in making the product safer. Of course, not all costs and benefits can be measured in monetary terms—the cost of a death, for instance, or the cost in foregone product utility by making a knife less sharp. Moreover, the "benefits" achieved in such circumstances are enjoyed only by those members of the population who would have been harmed had the standard not been in effect, while the costs are paid by all consumers of the product. In this sense, a safety standard based on cost-benefit reasoning is a form of compulsory insurance, whereby society as a whole profits, a few individuals (those who avoid injury or harm) gain a great deal, and many people lose a little bit.

A problem with thinking in terms of costs and benefits lies in estimating just how many injuries will be avoided by a given safety standard. It would be silly, for instance, to think that every bicycle injury could be eliminated by writing a bicycle safety standard. It is possible, however, to use the following rough estimating procedure: If it is known that 15 per cent of bicycle injuries result from brake failure, and if a brake standard is imposed, then it could be assumed that injuries will be decreased by 15 per cent, provided that the brake standard is 100-per-cent effective in eliminating brake failure.

Market imperfection arguments for product safety regulation, resting on concepts of imperfect information and involuntary risk-assumption, are most concerned with cases of product failure or malfunction. Cost-benefit analysis, on the other hand, makes little distinction between product failure—the handlebars falling off a bicycle—and what is called "foreseeable misuse," such as people climbing on the top step of household ladders despite labels warning that this is unsafe. A cost-benefit approach might call for a modification of ladders if the benefits of reduced injury justified the increased monetary costs, even though prudent ladder users would be forced to pay to protect imprudent ones. An analysis based on market imperfection might fail to see the justification for a mandatory standard, and argue instead for a consumer education campaign to inform more consumers about the correct use of household ladders. But in terms of costs and benefits, consumer education is probably the most expensive way, on a dollar basis, of achieving a given amount of accident reduction.

The bias of the CPSC

Bureaucrats in the CPSC are not unaware of these difficult questions. Agency officials are cognizant of the problems of increased

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costs and possible decreased utility; they think about concepts like freedom of choice and cost-benefit. Yet it is quite obvious that no way of looking at the question, "Should we set a mandatory standard for this product, and, if so, what should it look like?" will simply pump out objective answers. Inevitably, judgment plays a role—and so, therefore, do the officials' biases.

Decisions are easiest when market-imperfection analysis and cost-benefit analysis both point in the same direction. If it were to cost an extra half-cent per soda bottle to do additional testing that would decrease substantially the risk of the bottle exploding on the shelf when a customer picks it up, possibly blinding the victim, there are few people who would shy away from introducing a mandatory standard. If constructing children's blocks with smooth rather than sharp corners, thus reducing the risk of serious injury when children throw blocks at one another, were to cost a few extra pennies, few would object. On the other hand, if making bathtubs of a material that costs twice as much as porcelain made people only five per cent less likely to slip and fall in the shower, few would regard a mandatory intervention as justified.

But there is no way to avoid many hard cases. These become particularly acute in decisions to ban a product. How does one weigh the benefit to those who are able to use the product safely against the *involuntary risk of injury or death that other users face*?

Bureaucrats bring different sets of values and priorities to the decisions they make on such cases. In reading through working papers and talking with agency officials, one finds that some are hesitant about interfering with consumer choice while others have an almost puritanical aversion to letting people take voluntary risks. (Thus, a working paper on the dangers of snowmobiles moralizes, "Racing becomes a temptation and leads to collisions with other moving vehicles. . . . Dominating the environment may be a daredevil attitude on the part of many operators. The spirit of adventure guides drivers into uncharted fields and woods. Night riding is viewed as thrilling.") In a regulatory agency like the CPSC, uniformity of approach is supposed to be assured by the five commissioners, who must pass on all proposed standards, and who are briefed with papers which appear to set down fairly the arguments for and against.

In making their decisions, though, agency officials, including the commissioners, would seem to have a bias in favor of the agency's "doing something." The CPSC is not in charge of keeping down the cost of living, or saving people an ounce of inconvenience, or maxi-

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mizing freedom of choice (even though officials do take such factors into consideration). The CPSC is in the business of reducing injury from consumer products, and it is hardly surprising that its bias is towards taking measures which promote that goal, even when they have attendant costs.

The first victim of this bias is likely to be product utility, especially in cases where officials regard the utility of the product or product feature in question as minor or frivolous, regardless of whether it has achieved market acceptance. "If A and B both get dishes clean, but A burns out your eyes, why not take A off the market?" is the way one commissioner expressed this view. Of course, some people may buy A rather than B because they feel that A gets their dishes a *little* cleaner; a dogmatic believer in the verdict of the marketplace would presumably hold that all products accepted on the market are created equal—that a slight extra gleam on dishes is worth a hundred cases of children blinded for life. The rest of us are left to weigh conflicting factors on a case-by-case basis. Officials in charge of toy regulation state that if they are unsuccessful in attempts to develop a standard that will protect children from injury from electric toy irons or ovens, and if there is evidence that deaths do indeed occur, then they will seek to ban such toys, despite their popularity. (In the view of one non-governmental toy safety advocate, "110-volt household toys are perfectly unnecessary. A child should be permitted to use his or her imagination. They can provide the electric power mentally.") Another official, in charge of developing a bicycle standard, has moved to ban gear shifts along the bar of the bike because they can cause groin injuries if the rider falls down from the seat, even though such gear shifts are regarded as "sporty" by many riders.

Enter NEISS

Given the commission's bias in favor of doing something, it becomes important to know how it selects products to be regulated, and what it knows about the extent and nature of injuries associated with the thousands of consumer products under its jurisdiction. The Presidential study commission on product safety threw around figures like 20 million injuries requiring medical care and 30,000 deaths occurring annually within the home. But these figures were criticized as being little more than guesses based on a small survey by the Public Health Service in 1960, and they were misleading in that a large proportion of such injuries were associated with falling down stairs,

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playing football, or using kitchen knives—activities that federal regulations are not likely to affect much.

Today, the CPSC relies on an extremely sophisticated information-gathering system called the National Electronic Injury Surveillance System (NEISS), which was developed by William White, a former Public Health Service official, when he served as executive director of the product safety study commission. NEISS draws upon data from 119 hospital emergency rooms, selected to make up a statistically valid weighted probability sample of the American population. Every day these hospitals teletype reports to the CPSC's computer in Washington on the day's emergency room admissions involving product-related injuries. Recognizing the trade-off between quantity and accuracy of information, CPSC decided that these daily reports would contain data about only the type of injury, the body part injured, the product associated with the injury, and the age and sex of the victim. The Bureau of Epidemiology of the CPSC gets a computer printout each morning, from which it selects a certain number of cases to be assigned to in-depth investigators at the local level. The investigators go to the injured person's home, find out the accident sequence, and hopefully obtain pictures or samples of the product.

The nightly information sent to the NEISS computer is coded by special hospital employees. Product categories (some 850 of them) and the severity ratings (on a scale from 1 to 8) for some 250 injury categories are taken from NEISS codebooks. To allay physician fears, the NEISS computer never receives names or addresses of patients. The hospitals are paid about 60¢ for each injury they report, a sum that is intended just to cover their costs.

The most valuable information that can be calculated from the NEISS reports concerns the frequency and mean severity of injuries associated with various product categories. (To calculate mean severity, one simply adds up the severity values for each injury reported, and divides by the number of injuries.) But there are problems—as the agency realizes—both with the NEISS product categories and with the way severity is computed.

NEISS product categories are incredibly detailed. Thus, day clothing and night clothing are two separate categories, as are tables and desks. Injuries associated with a certain product category are listed together whether or not they have similar origins—a television set can explode or fall on someone's toe; one can slip on a carpet or ignite it by dropping a cigarette. If several products are involved in an injury sequence (a woman slips on a kitchen rug

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and ignites her nightgown on a gas stove) NEISS codes the injury under all three products, although they may play widely different causal roles. The CPSC is in the process of dealing with the problem of overspecification of product categories by going over the NEISS classifications and collapsing them in certain cases. But other problems are harder to get at.

Injury severity, as reported to NEISS by hospital coders, is rated on a scale from 1 to 8 by a methodology developed by scientists on the product safety study commission. To get ratings for the severity of different injuries, epidemiologists drew up a matrix crossing injury types (amputations through dermatitis) with body parts. The 250 combinations in the matrix (like leg fracture or arm laceration) were submitted to a number of health analysts, including a doctor, who were asked to rate them in severity from 1 to 6. Injuries like "strain or sprain, foot" or "dermatitis, hands" emerged from the combined medical judgment with the lowest severity ranking, number 1. Injuries like "amputation, 25-50 per cent of body" or "electric shock, all parts of body" were ranked 6. One point is added to the severity rating of any injury where the victim requires hospitalization. Thus if a sprained foot required hospitalization, it would become a 2 rather than a 1. Finally, the category 7 was reserved for injuries of severity 6 which require hospitalization, and the category 8 for victims dead on arrival.

It was felt, however, that this 1-to-8 coding scale did not reflect the true relative severity of different injuries. Few would dispute that one death is more serious than eight sprained ankles. But how much more serious? One way to weight relative severity might be to base it on average medical and lost worktime costs for different injuries. But this would have required complicated calculations, and still would not have dealt with the severity rating of a death. Instead, statisticians developed a non-linear numerical progression, which starts arbitrarily at the number 10 (corresponding to severity category 1) and, through the wonders of algebra, attains the fantastic number of 34,721 (corresponding to the severity category 8). These numbers, as one commissioner puts it, have "an algebraic rationale, but not a real-life one," and thus could cause problems for the agency. A number of officials believed that the enormous value for category 8 (the value for category 7 was only 2,516) skewed severity ratings because one death associated with a product, even if in a freak accident, might increase mean severity dramatically. In July 1973 an inter-bureau task force within the agency voted to combine categories 7 and 8. The effect of this is to change the severity weight

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for a death from 34,721 to 2,516. "This is a temporary solution," an agency memo states, "to be replaced by a means to utilize category 1 through 7 in the mean severity computations and probably treat the death event separately from mean severity."

These problems are important for the CPSC because, as White envisioned it, NEISS statistics were to provide the basis for all CPSC priority determinations and policy evaluations. Product categories associated with high frequency or severity of injury would be at the top of the CPSC's list for new standards, and changes in the number and severity of injuries after the imposition of standards would be the primary test of the effectiveness of those standards. Furthermore, the agency would rely on the in-depth investigations, conducted as the second phase of NEISS, for guidance in determining the content of standards.

The uses of in-depth investigation

If one asks agency officials what guides them in determining the features appropriate for a product standard—why they fiddle with one aspect of the product but not another—the answer will be something like: "We follow the accident pattern as revealed in in-depth investigations." What this means is obvious in some cases. If one looks at a pile of in-depth investigations for power lawnmower accidents, it quickly becomes apparent that almost every lawnmower injury results from one of two causes: The victim is injured by a moving blade while putting his hand into the grass injection shoot to clean it out, or a rock or wire is ejected out of the blade at high speed against the victim's foot. From reading in-depth investigations reports, the standard setter would conclude that a power mower standard should deal with the discharge chute and the absence of a back guard to deflect stones. Similarly, in-depth investigations showing toy injuries from protruding parts in toy rifles would suggest a standard against protruding parts.

Interpreting the evidence provided by in-depth investigations in this way, however, is not always so straightforward; it often involves bureaucratic discretion, and in some instances can come perilously close to crystal-ball gazing. I listened to the official in charge of developing a bicycle standard explain that each feature of the standard he was writing was intimately related to a hazard pattern in the in-depth investigations, and that no more and no fewer features for the standard were justified by the "evidence." But I was not completely convinced.

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The commission sets priorities for in-depth investigations, which in turn are programmed into the computer so that each morning's printout indicates which of the injuries reported that day deserve looking into. In general, in-depth investigations are indicated for all victims dead on arrival (DOA's), injuries involving products already subject to standards, and injuries involving product categories where the CPSC is actively considering new standards. When such injuries have taken place in locations within a reasonable distance of a CPSC field office (or a private contractor the commission uses), a request for an "in-depth" is sent out. Field investigations are supposed to take place within 72 hours. When work on standard writing begins, CPSC epidemiologists compose "hazards evaluation reports" summarizing the hazard patterns revealed in the in-depth investigations.

In-depth investigations are also supposed to be of some help in enforcement and in picking out new and perhaps dangerous products that the commission otherwise wouldn't know about. Normally, in-depth investigations are filed away until the time has come to write hazard analysis reports. But before being filed, investigations (which are supposed to include specific brand names and stock numbers when available) are checked for possible violations of existing standards. If such violations are found, a copy of the report is sent to CPSC compliance officials.

Part of the rationale for in-depth investigations of all DOA's is that they may uncover new or unusual products that might be dangerous. Through a DOA electrocution case reported to the old Bureau of Product Safety, officials learned about a product called an "electric worm probe," which used a copper rod to shoot an electric impulse into the ground in order to bring up night crawlers. The manufacturer agreed voluntarily to stop selling the product.

"Priorities"

"The Consumer Product Safety Commission's goal is to reduce the number of deaths and injuries resulting from the use of everyday products," stated the budget request justification the CPSC prepared for the House Appropriations Committee for FY1973. In his testimony, Richard Simpson, chairman of the commission, added, "I think it is wrong only to report the number of standards issued and only report the number of penalties imposed, et cetera. I think if we are really doing our job, we ought to honestly say how many injuries did we reduce and further how much did it cost the American consumers."

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Within the short period of the agency's existence, this way of defining the commission's goal has become so ingrained in the agency's self-image that it is nearly impossible to speak with an agency official without constantly hearing the word "priorities." The CPSC, one hears, must go after products in an order determined by some objective criteria of how dangerous they are. This, in the agency's view, helps not only in achieving its goal—maximizing injury reduction—but also in protecting the agency against public criticism: "If you can't justify your decision to go after one or another kind of product in an objective way," a high CPSC official states, "the agency will constantly be attacked because it's not working on some problem somebody considers important."

The word "priorities" has become such a part of agency style that all methods of determining commission actions on the basis of what they call "oiling the squeaky wheel"—responding to consumer complaint letters, Congressional inquiries, newspaper campaigns, and the like—are shrugged off with derision. The Consumer Product Safety Act, for example, contains provisions allowing private citizens or consumer groups to petition the commission to adopt standards for a product, but the official agency view is that petitions regarding items not on the agency "priority list" will be rejected on the ground that other items the agency is working on are more important. As for consumer complaint letters (which many outsiders probably think of as the main goad for an agency of this sort), they land on the desks of various bureaus within the agency according to a mysterious sorting system. But it seems fair to state that they are usually lost in a flurry of inter-bureau routing slips. "Without a logical procedure," writes an intra-agency paper, "complaints often force illogical priorities. . . . I do not believe that citizen concerns, in and by themselves, provide the basis for a priority system."

Three employees of the old Bureau of Product Safety, John Locke (who served briefly as CPSC executive director before leaving), Stan Parent (now special assistant to Chairman Simpson), and Ted Woronka (now with the commission's Office of Program Planning, Budget, and Evaluation) are responsible for the agency's priorities orientation. "In a regulatory agency," says Parent, "it has always been very difficult to predict what the flaps would be—what items would require regulatory attention. If you can't predict, then you can't budget. The only thing left to do is to structure your organization so as to sharpen your ability to respond quickly. This is what we tried to do in the old Bureau of Product Safety. We built a quick-response system by having weekly management meetings to allocate contract

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money and agency skills. We reordered our priorities every week, so to speak."

Any priority system must have as its starting point data on accident frequency and severity, so it was not until William White developed NEISS that Parent and his associates began thinking in terms of how to develop a priority system for the yet unborn new agency. In the hiatus between the passage of the Consumer Product Safety Act and the formation of the new commission, John Locke wrote a paper entitled "Identifying Product Safety Priorities" for presentation at a professional meeting. Shortly after Richard Simpson was appointed commission chairman, he met with Stan Parent to get the latter's ideas on two bureaucratic problems which bothered him—making agency decisions more objective and less arbitrary, and preventing various bureaus in the agency from "doing their own thing" just to make work. Parent showed Simpson the Locke paper, and Simpson's reaction was that a priority system provided an answer to most of his questions on how to run a regulatory agency. Priorities make decisions less arbitrary. They also give each bureau within the agency a sub-task that fits into the whole. Writing a standard requires epidemiological, economic, and engineering information. With a priority system, the Bureaus of Epidemiology, Economic Analysis, and Engineering Science have as their tasks the feeding of information on product categories being dealt with to the "project coordinator" in the Office of Standards Coordination and Appraisal. Simpson quickly became a convert.

The risk index

Locke's paper begins by discussing various ways in which a priority system could be set up. At the simplest level, one could set a priority list based on frequency of injury (most frequent categories first) or severity of injury (worst categories first). These two methods give utterly different priority lists. Products most frequently associated with injuries include stairs, bicycles, beds, pens and pencils, and razors; those associated with the severest injuries include power mowers, cribs, household chemicals, and nightwear.

But in addition to matters of injury frequency and severity, questions like the increased cost of the proposed safety features, the likelihood of a standard's reducing injuries, and voluntary vs. involuntary risks are important in priority setting. A systems analysis approach might combine frequency and severity of injury with estimates of the cost of change in such a way as to maximize the quantity

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of injury reduction per dollar spent. But computations of this sort would require major economic cost studies on wide ranges of product categories *before* decisions to go ahead with any standard.

Locke concluded that two composite measures should be used in setting priorities. The first, the "risk index," is an interesting attempt to get an acceptable quantitative way of measuring the risk of products—that is, how likely it is that one will be injured using a given product. Locke's formula is as follows:

$$\text{Risk index} = \frac{\text{injury frequency x injury mean severity}}{\text{exposure}^1}$$

According to the risk index, if two products were each associated with the same frequency and severity of injury, but one was found in every household (say, a bed) while the other was relatively arcane (say, a fondue pot), the arcane product would come up higher on the risk index because using the product is more dangerous in the sense that injury is more likely to occur during any single use. It is easy to see that a product might be associated with large numbers of injuries of high severity, and still come out low on the risk index simply because it was in such common use.

There are difficulties with the calculation of exposure in the risk index. Statistics for the number of products in use often do not exist (Locke was able to locate them for only 27 of the 60 product categories which topped his frequency and severity lists). Furthermore, exposure cannot be measured simply by assessing the number of products in circulation. We sleep in a bed eight hours a day, we wear a shirt 14 hours a day, we use a razor for two minutes a day. Some products we use daily, others only once or twice a year.

But there is also a deeper controversy around the very idea of setting priorities based on a risk index. *Risk reduction* is one philosophy of the agency's role, but it is not the one expressed in the commission's official version of its goal: "to reduce the *number* of deaths and injuries" from consumer products. A risk-based priority system would aim first at products that are most dangerous, even if they were responsible for only a few injuries. An alternative philosophy, *casualty reduction*, comes closer to the agency's stated goal. A casualty-based priority system would be founded on a simple index of frequency and severity, without any consideration of exposure.

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[1] Exposure relates to the extent of consumer contact with various products. In his paper, Locke chose to define it as approximated by the number of those products in use.

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Locke also constructed a second measure, the "subjective factors index." He took three factors—"likely success of a standard in reducing injury," "cost to eliminate hazard," and "cost of injuries prevented"—and rated each on a 1-to-3 scale for each of the products ranking high on his risk index. He arbitrarily subtracted 1 from the total subjective factors rating if the risk was involuntary. This procedure gave a number value, and the lowest numbers became highest-priority items according to the subjective factors index.

Selecting a priorities scheme

Just after CPSC officially came into existence, Locke, at that point executive director of the commission, appointed a task force of agency bureau heads to explore different methodologies for establishing a priority list. The main purpose of the task force was to argue out questions like risk reduction versus casualty reduction as an agency philosophy, and the role of subjective factors in a priority list; another purpose was to gain the bureau heads' commitment to a priority-based approach to the agency's activities. After two months of weekly meetings, the task force prepared for the commissioners' consideration a list of the alternative measures that might be chosen as the basis of a priority scheme:

1) frequency of injury
2) severity of injury
3) frequency-severity index (FSI)
4) age-adjusted FSI (accidents occurring to the very young and very old counted double)
5) risk index
6) subjective factors-adjusted index (the number obtained for each product category by any of the methods above would then have 10 per cent—or, alternatively, 50 or 100 per cent—of its value added or subtracted depending on whether the risk was voluntary or involuntary, the likelihood of injury reduction great or small, and the cost of change small or large)
7) a composite index (such as a summation of values on the risk index and the FSI)

I was present at the commissioners' meeting at which the task force presented its report. Two of the commissioners were concerned about the way the severity scale was calculated in the NEISS system. Why was a broken arm in category 3 rather than category 4? What was the logic of the geometric curve leading from 10 to 34,721? Could the system be explained to the public? The most consistently asked

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kind of question from the commissioners was "Where did you get that?" or "What's the justification for that?"

The task force presented the commissioners with computer printout lists of the effects on the "top 30 products list" of the various alternatives they proposed. The frequency-severity index, they said, seemed to be more sensitive to frequency than to severity of injury. Giving the subjective factors a weight of 10 per cent had hardly any effect on the list, and even giving it a weight of 50 per cent moved relatively few items around. The task force chairman concluded by saying that there was sentiment on the task force for four of the alternatives: the risk index, the age-adjusted FSI, and either of these with subjective factors weighted alternatively at 10 per cent and 50 percent. Most members of the task force ideally favored a risk-based approach, but felt it to be impossible with currently available exposure data. They therefore favored the use of the age-adjusted FSI as the second-best choice; by a narrow margin the members were also against using the subjective factors on the first-pass priority list, feeling that such ratings would be too subjective, and the weighting among subjective factors too arbitrary. Things like the cost of change and the likelihood of injury reduction could then be measured more precisely when the product came up for standard setting.

"Look," interrupted Simpson, the chairman of the commission, "if something is high on the list, there's always *something* we can do, even if it's not a standard. Maybe no standard is practical. Then we could have an educational campaign."

After some further discussion, Simpson said to the chairman of the task force, "OK, so you want to go with the age-adjusted FSI?"

"Well, we didn't want to make any final recommendation. We wanted to present you with the arguments, and let you decide."

"But most of you favor the age-adjusted FSI?" Simpson pressed.

"Well, we'd like to see something better. If we had more time, we might be able to develop something better."

"Any company designer can always improve the design of his product the more time he's given. But at some point the manufacturer wants to get the product on the market. We are in a similar situation with wanting to get a list out on the market." Simpson then directed the task force to make a definite recommendation to the commissioners at the next meeting.

To meet the request, the task force polled all bureau and office directors and the division directors under them on their first, second, and third choices for a priority system. The poll gave the highest number of votes to the age-adjusted FSI, and, at a second meeting,

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the commissioners voted to accept this as the system for commission priorities.

Any list is better than no list

On September 28, 1973, the CPSC published a ranking of almost 400 product categories (which involved collapsing some of the original NEISS categories). The "10 most wanted" products on the list, and their age-adjusted FSI ratings, were:

1)	bicycles	863,490
2)	stairs	833,120
3)	doors (non-glass)	389,950
4)	cleaning agents (caustics)	386,310
5)	tables	369,990
6)	beds	304,890
7)	football activity	296,700
8)	swings, slides	287,260
9)	liquid fuels	267,150
10)	architectural glass	267,100

Thus, perhaps not surprisingly, the "top 10" products include only three or four (bicycles, cleaning agents, architectural glass, and perhaps swings and slides) that the commission is likely to be able to do much about. If one goes into the top 30, there are a number of products which are "old faithfuls" in discussions of product safety—lawnmowers, space heaters, cooking ranges, clothing, household chemicals, matches, ladders. (To get an idea of the numbers involved in the age-adjusted FSI, a product like dolls and toy animals, ranked number 199, has an age-adjusted FSI of 4,180.)

After the commissioners' vote, John Locke stated that the task force still viewed a risk-based system as better than the casualty-based system whose adoption had just been recommended and accepted. "We hope to continue working on the exposure problem," he said. "The problem is that no outside contractors have much credibility or experience in working with this type of problem. We could end up paying some contractor to do research and come to the point where we are already." Some members of the task force complain that the result of their work was merely a least-common-denominator solution that avoided the interesting problems. But a bureaucratic agency is not a think tank, and the commissioners appear to be satisfied. "Maybe it's not productive to try to be perfect when we're better than everybody else," one told the members of the task force. Another said, "I'd rather have a list telling us what to do than have

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Magnuson or Percy telling us what to do. Any list is better than no list."

The government does already have something of a track record on product safety regulation. Although there is no space to detail it here, I think it is fair to say that the development of standards for the flammability of children's sleepwear, carpets, and mattresses was done professionally and well. The development of childproof packaging for household poisons like drugs, caustics, and furniture polish represented an imaginative solution to a regulatory dilemma of balancing the needs and desires of those who find products necessary and desirable, and the interests of children, several hundred of whom are poisoned to death each year. (Poisoning is one of the largest accidental causes of death for children under five, and there is some empirical evidence that poison prevention packaging has a significant effect in reducing the number of deaths.) One of the most impressive aspects of the regulatory effort by the predecessor agencies to the CPSC was the development of government expertise in fields of product safety equal to or greater than that possessed by the industries to be regulated.

The CPSC is a new agency, and therefore has a surfeit of unusually energetic and devoted officials working for it. It can be assumed that as time passes the agency will come to be inhabited by a more normal run of officials, some good, some bad. It has been said that a good organization is a system designed by a genius, that can be run by idiots. Simpson and his staff have attempted to design their organization from the beginning so that its goal is clear and its method of standard setting minimizes arbitrariness. This is what political scientists have always asked heads of new agencies to do. Now one has. It will be interesting to see what difference it makes.

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Does Punishment Deter Crime?

GORDON TULLOCK

TRADITIONALLY there have been three arguments for the punishment of criminals. The first of these is that punishment is morally required or, another way of putting the same thing, that it is necessary for the community to feel morally satisfied. I will not discuss this further. The two remaining explanations are that punishment deters crime and that it may rehabilitate the criminal. The rehabilitation argument was little used before about 1800, presumably because the punishments in vogue up to that time had little prospect of producing any positive effect upon the moral character of the criminal.[1] *

But with the turn to imprisonment as the principal form of punishment—a movement which occurred in the latter part of the 18th and early part of the 19th century—the idea that the prison might "rehabilitate" the prisoner became more common. The word "penitentiary" was coined with the intent of describing a place where the prisoner has the time and the opportunity to repent of his sins and resolve to follow a more socially approved course of action after his release. The idea that prisons would rehabilitate the criminal and that this was their primary purpose gradually replaced the concept of deterrence as the principal publicly announced justification for the punishment system. I should like to defer discussing my views as to why this occurred until the latter part of this article, but here I should like to point out that, whatever the motive or the reason for this change, it certainly was not the result of careful scientific investigation.

So far as I have been able to discover, there were no efforts to test the deterrent effect of punishment scientifically until about 1950. At that time, several studies were made investigating the question whether the death penalty deterred murder more effectively than life imprisonment. These studies showed that it did not, but they were extremely primitive statistically. This is not to criticize the scholars who made them. Computers were not then readily available, the modern statistical techniques based on the computer had not yet been fully developed, and, last but by no means least, the scholars who undertook the work were not very good statisticians. Under the circumstances, we cannot blame them for the inadequacies of their work, but neither should we give much weight to their findings.

*All footnotes may be found at the end of this article.

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Moreover, even if it were the case that the death penalty did not deter murder, it would not automatically follow that deterrence does not work in general. The argument is frequently made that life imprisonment is actually a more severe punishment than the death penalty and it might turn out to be true—at least in the eyes of potential murderers. If this were the case, then one would anticipate that life imprisonment would have a greater deterrent effect than would execution. But in any event, the findings obtained in these early studies were largely the result of their very primitive statistical techniques.

Statistically testing deterrence is not easy because the prospect of punishment obviously is not the *only* thing that affects the frequency with which crimes are committed. The crime rate varies with the degree of urbanization, the demographic composition of the population, the distribution of wealth, and many other circumstances. Some statistical technique is necessary to take care of these factors—and such techniques are now available. Using multiple regression (or, in a few cases, a complicated variant on the Chi-Square test), it is possible to put figures on each of these variables into the same equation and to see how much they influence the dependent variable which, in this case, is the rate of a specific crime. Although there are difficulties, this procedure will give a set of numbers called coefficients that are measures of the effect of *each* of the purported causative factors on the rate of commission of the given crime. If punishment deters crime, it will show up in these figures as a coefficient that is both significant and negative. A number of other things in the equation may also show up as affecting the crime rate, but the purpose of this article is to discuss only whether *punishment* does or does not deter crime.

One of the basic problems with any kind of statistical research in the field of criminology is the appallingly poor quality of the data. Any study will have a great deal of what the statistician calls "random noise" in it. Most of the studies mentioned below use the FBI's *Uniform Crime Report* statistics, and almost all of the authors have made comments about how bad these statistics are. I am happy to say that the Law Enforcement Assistance Administration has begun a project aimed at a sharp improvement in crime statistics, and hence we can anticipate that such research will be a great deal easier in the future. All of the studies I will report are based on the earlier and poorer statistics, but in about a year or so there should be a new generation of studies drawing upon the much better data that will be available at that time.

THE recent studies in deterrence come partly from economists and partly from sociologists. As an economist myself, I may be pardoned for starting with the economic studies, but I should say that, due to the long delay that intervenes between research and publication, it is not at all obvious which discipline actually had priority.

Most economists who give serious thought to the problem of crime immediately come to the conclusion that punishment will indeed deter crime. The reason is perfectly simple: Demand curves slope down-

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ward. If you increase the cost of something, less will be consumed. Thus, if you increase the cost of committing a crime, there will be fewer crimes. The elasticity of the demand curve, of course, might be low, in which case the effect might be small; but there should be at least *some* effect.

Economists, of course, would not deny that there are other factors that affect the total number of crimes. Unemployment, for example, quite regularly raises the amount of crime and, at least under modern conditions, changes in the age composition of the population seem to be closely tied to changes in the crime rate. The punishment variable, however, has the unique characteristic of being fairly easy to change by government action. Thus, if it does have an effect, we should take advantage of that fact.

The 19th-century utilitarians had drawn this conclusion, and when economists in the 1950's and early 1960's began turning their attention to the problem of deterrence, this rather simple application of economic theory was one of the first things that occurred to them.[2] The first econometric test of this theoretical deduction from economics was performed by one of Gary Becker's graduate students, Arleen Smigel Leibowitz, in her Master's thesis.[3] The basic design of this research project was reasonably sophisticated, although, as can be seen below, it has been improved upon since then. Leibowitz used as her basic data the crime rate and the punishment for a number of different crimes in each state in the United States. She took into account both the severity of punishment (i.e., the average prison sentence) and the probability that punishment will actually be imposed (i.e., the percentage of crimes whose perpetrators are caught and sent to prison). A number of essentially sociological factors that might affect the crime rate were also included in her multiple regressions. Leibowitz's findings revealed an unambiguous deterrence effect on each of the crimes studied—that is, when other factors were held constant, the states which had a higher level of punishment showed fewer crimes. Such crimes as rape and murder were deterred by punishment just as well as (indeed, perhaps better than) burglary and and robbery.

Another of Becker's students, Isaac Ehrlich, in his doctoral dissertation went over much the same ground as Leibowitz but with a much more sophisticated and careful statistical methodology. The results, which are available in full text in his dissertation and in a somewhat abridged form in an article,[4] once again indicate that punishment does deter crime.

Further work along the same general lines was carried out by Llad Phillips, Harold L. Votey, Jr., and John Howell. In general, these scholars used the same basic data and analytical methods as Leibowitz and Ehrlich, and confirmed their findings. More recently, this group of scholars has used the same data and similar methods in an effort to produce more detailed and specific results.[5] These studies, which are of great interest in themselves, are relevant to our present purpose only in that, as a sort of by-product, they contain further con-

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firmation of the basic finding that punishment does deter crime. Further, Phillips has run a time-series test using national data in his multiple regression to supplement the cross-sectional tests on state data.[6] It also produced similar results. Last along this particular line, Morgan Reynolds in his doctoral dissertation has treated the same basic research design en route to some new results in another area.[7]

In addition to these studies using essentially the same data on crime and punishment in the 50 states, there are two important studies using different data. Michael Block compared the crime rates for Los Angeles police districts with the likelihood in each of these districts that offenders would be caught and sent to prison, and found a clear deterrence effect.[8] And R.A. Carr-Hill and N.H. Stern carried out a study using data drawn from England and Wales and, once again, determined that punishment does deter crime.[9]

Joseph Magaddino and Gregory Krohm, using California county data, have begun work which, from the results shown by their first regressions, apparently will lead to the same conclusion.[10] David Sjoquist and Phillips, Votey, and Donald Maxwell investigated somewhat different problems, but their statistical outcomes provide further support for the deterrence theory.[11]

Finally, some students under my direction attempted to make a cost-benefit analysis of certain property crimes, primarily burglary, from the standpoint of the criminal—that is, they looked into the question of whether crime does pay. The data were particularly bad in this area, as the reader can well imagine, but they supported the conclusion that most people who took up the profession of burglary had made a sensible career choice. They did not make very much from burglary, but they were not very high-quality laborers and would have done as badly (or worse) if they had elected honest employment.[12] This is not of direct relevance to the deterrence hypothesis, but it does seem to indicate that at least some criminals make fairly rational decisions with respect to their careers, and hence that raising the price of crime would presumably reduce the frequency with which it is committed.

Recently this point of view has been questioned by a short study by Michael Sesnowitz.[13] (The article was commented upon by Krohm and a reply was made by Sesnowitz.[14]) Following an approach rather similar to that used by my students, Sesnowitz found that burglary did not pay in Pennsylvania. Basically, the difference between Sesnowitz's results and those which I would have expected comes from the fact that there are no data on the amount of time served by burglars in Pennsylvania who are sentenced to jail rather than prison. Sesnowitz assumed that the average jail sentence was the same as the average prison sentence for burglary (43 months). But since it is illegal in Pennsylvania for anyone to spend more than 23 months in jail (as opposed to prison), it is most unlikely that this is so; and if adjustments for this discrepancy are made, the results wind up rather similar to those obtained by my students. Incidentally, Pennsylvania apparently does have an exceptionally high punishment level for

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burglars—and, correspondingly, an exceptionally low rate of burglaries, just as the deterrence hypothesis would predict.

So much for the economists; let us now turn to the sociologists. All the economists I have cited began their studies under the impression that punishment *would* deter crime. All the sociologists I am about to cite began under the impression that it *would not* and, indeed, took up their statistical tools with the intent of confirming what was then the conventional wisdom in their field—that crime cannot be deterred by punishment. When they found out they were wrong, they quite honestly published their results, although they found it rather difficult to get their work accepted in the more conventional sociological journals.

The first of these sociologists was Jack Gibbs, who published a study in the *Social Science Quarterly* which indicated that punishment did indeed deter crime.[15] His statistical methods were basically rather different from those used by the economists—indeed, speaking as an economist, I would say they were more primitive; but the fact that the same conclusion comes from two different statistical techniques is further confirmation of its validity. The publication of this paper set off a spate of other papers by Louis Gray and David Martin, Frank Bean and Robert Cushing, and Charles Tittle.[16] All of these scholars took up their cudgels with the intention of demonstrating that Gibbs was wrong, and all ended up agreeing with him. In the process, they greatly expanded and improved upon his work. Moreover, they continued using statistical tools that were somewhat different from those that had been employed by the economists; hence, their work can be taken as an independent confirmation of the economists' approach.

The sociologists were very much interested in a problem that had also concerned the economists, but not so vitally. This is the question whether the severity of the sentence or the likelihood that it will be imposed is more important in deterring crime. In my opinion, this is not a very important question. Suppose a potential criminal has a choice between two punishment systems: One gives each person who commits burglary a one-in-100 chance of serving one year in prison;[17] in the other there is a one-in-1,000 chance of serving 10 years. It is not obvious to me that burglars would be very differently affected by these two punishment systems, although in one case there is a heavy sentence with a low probability of conviction, and in the other a lighter sentence with a higher probability of conviction.

I would suggest that the appropriate technique is simply to divide the average sentence by the frequency with which it is imposed, and to use that as the deterrent measure. Most of the sociologists and a good many of the economists mentioned above have attempted to determine which of these two variables is more important. Leaving aside my theoretical objections, I do not think the statistics are accurate enough for the results obtained from these tests to be of much value. Be that as it may, more often than not the researchers have

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found that the frequency with which the punishment is applied is of greater importance than its severity.

THE first studies in this field, the ones I criticized at the beginning of my survey of the empirical literature, dealt with the death penalty. Recently Ehrlich has returned to this problem and, by using a much more sophisticated method, has demonstrated a very sizeable deterrence payoff to the death penalty for murder.[18] His figures indicate that each execution prevents between 8 and 20 murders. Unfortunately, the data available for this study were not what one would hope for, so not as much reliance can be put upon his results as one normally would give to work by such a sophisticated econometrician. Earlier, and using a quite different set of statistics and a different method, I arranged to have a graduate student do a preliminary study of the same issue; his results showed that each execution prevented two murders. Here again, however, the data were bad and the methods were suitable only for a preliminary exploration.[19]

It should be emphasized that the question of whether the death penalty deters murder is a different one from the question of whether we wish to have the death penalty. One widespread minor crime is failing to return to the parking meter and put in a coin when the time expires. I take it that we could reduce the frequency with which this crime is committed by boiling all offenders in oil. I take it, also, that no one would favor this method of deterrence. *Thus, the fact that we can deter a crime by a particular punishment is not a sufficient argument for use of that punishment.*

In discussing the concept of deterrence, I find that a great many people seem to feel that, although it would no doubt work with respect to burglary and other property crimes, it is unlikely to have much effect on crimes of impulse, such as rape and many murders. They reason that people who are about to kill their wives in a rage are totally incapable of making any calculations at all. But this is far from obvious. The prisoners in Nazi concentration camps must frequently have been in a state of well-justified rage against some of their guards; yet this almost never led to their using violence against the guards, because punishment—which, if they were lucky, would be instant death, but was more likely to be death by torture—was so obvious and so certain. Even in highly emotional situations, we retain some ability to reason, albeit presumably not so well as normally.

It would take much greater provocation to lead a man to kill his wife if he knew that, as in England in the 1930's, committing murder meant a two-out-of-three chance of meeting the public executioner within about two months than if—as is currently true in South Africa —there were only a one-in-100 chance of being executed after about a year's delay.[20]

Another example can be drawn from the American South. Before about 1950, there was a great deal of violence among blacks, particularly on Saturday nights. The local authorities took the view that this was an inherent matter of black character, and hence were

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reluctant to punish it severely. It has been pointed out that the reluctance of the police to punish such "black" traits was probably the principal reason for their existence. A black who slashed another black's face with a razor on Saturday night would probably merely be reproved by the police and, at most, would get a short term in jail. A white who did the same thing to another white would probably get several years in prison. The difference between the statistical frequency with which blacks and whites performed this kind of act is thus explicable in terms of the deterrence effect of punishment as it was then administered.

It should be noted that thus far I have said nothing whatsoever about how well-informed criminals or potential criminals are as to the punishments for each crime in each state. For punishment to have a deterrent effect, potential criminals must have at least some information about its likely severity and frequency. Presumably, the effect of variations in punishment would be greater if criminals were well-informed than if they were not. In practice, of course, potential criminals are not very well-informed about these things, but they do have some information.

Reports of crimes and punishments are a major part of most newspapers. It is true that most intellectuals tend to skip over this part of the newspaper, but the average person is more likely to read it than some things that appeal to intellectuals. And an individual who is on the verge of commiting a crime or has already taken up a career of crime is apt to be much more interested in crime stories than is the average man. He should have, therefore, a rough idea of the severity of punishments and of the probability that they will be imposed. This information should affect the likelihood that he will choose to commit a given crime.

Nevertheless, the information that he will have is likely to be quite rough. Undoubtedly, if we could somehow arrange for people to have accurate information on these matters, we would get much better coefficients on our multiple regression equations for the deterrence effect of punishment. But since governments have a motive to lie—i.e., to pretend that punishment is more likely and more severe than it actually is—it is unlikely that we can do much about improving this information. Still, the empirical evidence is clear. Even granting the fact that most potential criminals have only a rough idea as to the frequency and severity of punishment, multiple regression studies show that increasing the frequency or severity of the punishment does reduce the likelihood that a given crime will be committed.

FINALLY, I should like to turn to the issue of why "rehabilitation" became the dominant rationale of our punishment system in the latter part of the 19th century and has remained so up to the present, in spite of the absence of any scientific support. The reasons, in my opinion, have to do with the fallacy, so common in the social sciences, that "all good things go together." If we have the choice between preventing crime by training the criminal to be good—i.e., rehabili-

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tating him—or deterring crime by imposing unpleasantness on criminals, the former is the one we would *like* to choose.

The Reverend Sydney Smith, a follower of the deterrence theory, said a prison should be "a place of punishment, from which men recoil with horror—a place of real suffering painful to the memory, terrible to the imagination . . . a place of sorrow and wailing, which should be entered with horror and quitted with earnest resolution never to return to such misery . . ." [21] This is an exaggeration. Our prisons do not have to be that bad; the deprivation of liberty in itself may be a sufficiently effective punishment. But in any case, deterrence necessarily involves the deliberate infliction of harm.

If, on the other hand, we can think of the prison as a kind of educational institution that rehabilitates criminals, we do not have to consciously think of ourselves as injuring people. It is clearly more appealing to think of solving the criminal problem by means that are themselves not particularly unpleasant than to think of solving it by methods that *are* unpleasant. But in this case we do not have the choice between a pleasant and an unpleasant method of dealing with crime. We have an unpleasant method—deterrence—that works, and a pleasant method—rehabilitation—that (at least so far) never has worked. Under the circumstances, we have to opt either for the deterrence method or for a higher crime rate.

FOOTNOTES

[1] Of course, they might prevent him from committing the crime by making it physically impossible. Cutting off both hands of a forger and hanging them about his neck probably had no effect on his desire to commit forgery, but certainly made it very hard to do.

[2] See Gary Becker, "Crime and Punishment: An Economic Approach," *Journal of Political Economy,* 76 (March/April, 1968), pp. 169-217. See, also, Gordon Tullock, "The Welfare Costs of Tariffs, Monopolies, and Theft," *Western Economic Journal,* 5 (June, 1967), pp. 224-32; and "An Economic Approach to Crime," *Social Science Quarterly,* 50 (June, 1969), pp. 59-71.

[3] Arleen Smigel Leibowitz, "Does Crime Pay: An Economic Analysis" (unpublished Master's thesis, Columbia University, 1965).

[4] Isaac Ehrlich, "Participation in Illegitimate Activities: An Economic Analysis" (unpublished Ph.D. dissertation, Columbia University, 1970); and "Participation in Illegitimate Activities: A Theoretical and Empirical Investigation," *Journal of Political Economy,* 81 (May/June, 1973), pp. 521-65.

[5] Harold L. Votey, Jr. and Llad Phillips, *Economic Crimes: Their Generation, Deterrence, and Control* (Springfield, Va.: U.S. Clearinghouse for Federal Scientific and Technical Information, 1969); Harold L. Votey and Llad Phillips, "The Law Enforcement Production Function," *Journal of Legal Studies,* 1 (June, 1972); Llad Phillips and Harold L. Votey, Jr., "An Economic Analysis of the Deterrent Effect of Law Enforcement on Criminal Activity," *Journal of Criminal Law, Criminology, and Police Science,* 63 (September, 1972); and Llad Phillips and Harold L. Votey, Jr., "The Control of Criminal Activity: An Economic Analysis," in *Handbook of Criminology,* ed. by Daniel Glaser (Chicago: Rand McNally & Co., forthcoming).

[6] Llad Phillips, "Crime Control: The Case for Deterrence," in *The Economics of Crime and Punishment,* ed. by Simon Rottenberg (Washington, D. C.: American Enterprise Institute for Public Policy Research, 1973), pp. 65-84.

[7] Morgan Reynolds, "Crimes for Profit: The Economics of Theft" (unpublished Ph.D. dissertation, University of Wisconsin, 1971).

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[8] Michael Block, "An Econometric Approach to Theft" (Stanford University, mimeographed paper).
[9] R. A. Carr-Hill and N. H. Stern, *An Econometric Model of the Supply and Control of Recorded Offenses in England and Wales*, rev. (University of Sussex: School of Social Science, 1972).
[10] Joseph P. Magaddino and Gregory C. Krohm (untitled paper, in progress).
[11] See David L. Sjoquist, "Property Crime and Economic Behavior: Some Empirical Results," *American Economic Review*, 83, no. 3 (1973); and Llad Phillips, Harold L. Votey, Jr., and Donald Maxwell, "Crime, Youth, and the Labor Market," *Journal of Political Economy*, 80 (May/June, 1972).
[12] William E. Cobb, "Theft and the Two Hypotheses," in *The Economics of Crime and Punishment*, ed. by Simon Rottenberg (Washington, D. C.: The American Enterprise Institute for Public Policy Research, 1973), pp. 19-30; Gregory C. Krohm, "The Pecuniary Incentives of Property Crime," *idem*, pp. 31-34; and J. P. Gunning, Jr., "How Profitable is Burglary," *idem*, pp. 35-38.
[13] See Michael Sesnowitz, "The Returns to Burglary," *Western Economic Journal*, 10 (December, 1972), pp. 177-81.
[14] Gregory C. Krohm, "An Alternative View of the Returns to Burglary," *Western Economic Journal*, 11 (September, 1973), pp. 364-7; and Michael Sesnowitz, "The Returns to Burglary: An Alternative to the Alternative," *idem*, pp. 368-70.
[15] Jack Gibbs, "Crime, Punishment, and Deterrence," *Southwestern Social Science Quarterly*, 48 (March, 1968), pp. 515-30.
[16] Louis N. Gray and J. David Martin, "Punishment and Deterrence: Another Analysis of Gibbs' Data," *Social Science Quarterly*, 50 (September, 1969), pp. 389-95; Frank D. Bean and Robert G. Cushing, "Criminal Homicide, Punishment, and Deterrence: Methodological and Substantive Reconsiderations," *Social Science Quarterly*, 52 (September, 1971), pp. 277-89; and Charles R. Tittle, "Crime Rates and Legal Sanctions," *Social Problems*, 16 (Spring, 1969), pp. 409-23.
[17] This is actually somewhat higher than the risk that burglars now face in most parts of the United States.
[18] Isaac Ehrlich, "The Deterrent Effect of Capital Punishment: A Question of Life and Death." This is to be published in the *American Economic Review*.
[19] Since I cannot possibly claim to have read everything that has ever been written on the subject, I have been conducting part of my research in this area by asking people who hear my speeches or read my papers to tell me if they know of any other articles or books in which the effectiveness of deterrence has been tested in a reasonably scientific manner. I have never received a positive response to this question, but I repeat it here.
[20] These figures are for blacks killing blacks, not for blacks killing whites, or, for that matter, whites killing whites.
[21] Sydney Smith, *On the Management of Prisons* (London: Warde Locke and Company, 1822), pp. 226, 232.

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Extending the "No-fault" Idea

JEFFREY O'CONNELL

In his new book of essays, *Coping*, Daniel P. Moynihan has pointed out the merits of "thinking small" on the part of reformers. "After a period of chiliastic vision, we have entered a time that requires a more sober assessment of our chances, and a more modest approach to events. . . . [T]he here-and-now and the close-at-hand are the dominant facts of public life, and the proper study of those who would take part in it." As an exemplar of a relatively modest but effective social and economic form, Moynihan later cites (in an essay entitled "The Automobile and the Courts") the institution of so-called no-fault auto insurance laws.

No-fault auto insurance, which substitutes quick payment for acrimonious argument, has already been enacted into law in 14 states: Colorado, Connecticut, Florida, Georgia, Hawaii, Kansas, Kentucky, Massachusetts, Michigan, Minnesota, Nevada, New Jersey, New York, and Utah. Its merits are most easily seen in comparison with the system that it has replaced.

Under the old tort liability system, after an accident, A claims against B, basing his claim on the fact that he was free from fault and B was at fault. Because A is an "innocent" party claiming against a "wrongdoer," A is allowed to claim not only for his out-of-pocket loss but for the monetary value of his pain and suffering as well. But because of the difficulty of establishing not only who was at fault but the pecuniary value of pain (what *is* an aching arm worth?), payment is either nonexistent or, if at long last it does come through, often falls far short of the amount claimed. We all wind up shortchanged because expensive experts must be hired to haggle over who and how much is to be paid, and the wages of these experts drain the so-called insurance pool. In short, the tort insurance system is not a system for paying accident victims from accident insurance (as sensible as that idea would seem to be) but a system for *fighting* accident victims about whether to pay them from accident insurance.

Under the no-fault system, on the other hand, after an accident between A and B, each is paid regardless of who may be at fault, by his own insurance company, for his own out-of-pocket loss (rela-

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tively easy to total up from, say, his medical bills and lost wages).[1] As a corollary, each is required to waive his tort claim based on fault against the other. As a result of the savings from eliminating legal fees (no-fault insurance has been called "no-lawyer" insurance) and restricting payment to tangible losses, most people are eligible for payment from the insurance pool, into which fewer—or at least no more—dollars need be paid. This is, in essence, the "miracle" being wrought by no-fault auto insurance. It seems likely that the success of no-fault auto insurance laws—witness the successful experience in both Massachusetts and Florida, the first states to enact them—will eventually bring about higher no-fault benefits and a corresponding reduction in the number of claims based on fault that are allowed. The "miracle," then, may improve itself with time.

THE success of no-fault auto insurance naturally raises the question whether the no-fault concept may be extended to cover other kinds of accidents—for example, injuries resulting from manufactured products and from medical malpractice. In 1970, after over two years of investigation, the Presidential Commission on Product Safety released its Final Report, a 199-page document (supplemented by 16 fat supporting volumes) that includes an awesome civilian casualty list. Twenty million Americans are injured each year by consumer products alone, the Commission found.[2] Of these, 110,000 are permanently disabled, 30,000 killed. The annual costs to society from such injuries may well exceed $5.5 billion according to the Commission, of which more than $4 billion consists of "costs to the injured persons, their families, and close friends or relatives, whether or not the monies are reimbursed."

Despite such actuarially predictable carnage, the total amount of compensation available to victims from product liability insurance is negligible, even when compared with the notoriously meager benefits paid out to traffic victims from automobile liability insurance. For auto accidents, a U.S. Department of Transportation study found that, with compensable losses totalling $5.1 billion in serious injury and death cases, auto liability insurance paid only $800 million, or 15 per cent of compensable losses. For injuries caused by or related to consumer products, the percentage is apparently *much* lower. Precise figures are lacking here, but according to the Product Safety Commission product liability insurance reimburses "only a few per cent of the medical cost of injuries."

Meanwhile, getting the insurance companies to pay even that

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[1] The ceilings set on no-fault benefits can range from, say, $50,000 in New York State, where the law took effect February 1, to a low of $2,000 in Massachusetts.

[2] The Commission cites as the main culprits "a number of makes, models, or types" of 16 categories of products: "architectural glass, color television sets, fireworks, floor furnaces, glass bottles, high-rise bicycles, hot water vaporizers, household chemicals, infant furniture, ladders, power tools, protective headgear, rotary lawnmowers, toys, unvented gas heaters, and wringer washers."

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small percentage is likely to be exceedingly difficult. In the words of the Final Report, "[even in] the best of circumstances, a products [liability] case is still a bruising, frequently heartbreaking, always onerous undertaking for client and lawyer." The main reason is, of course, that in order to impose liability on a manufacturer for injury caused by his product, the product must be proven in some way defective, a process which almost always involves engineering evidence and testimony of the most technical kind. And while the requisite lawyers and engineers are huddling, conferring, and appearing in court, a very expensive meter is ticking.

Consequently, the Final Report states, "most injuries to consumers [resulting from the use of manufactured products] go uncompensated." Indeed, in light of how difficult it is to prosecute a products liability claim, the Report comments that "it is small wonder that some manufacturers do not even respond to letters claiming compensation for injuries: They know that more than two thirds will never pursue the claim. A survey of 276 persons in Denver and Boston who had reported injuries to Food and Drug Administration survey teams showed only four per cent contacted an attorney to investigate possible legal redress. And of those four per cent, less than one per cent (two out of 276) did so with intent of claiming against the manufacturer. In sum, says the Commission, "to advise a battered consumer to sue may simply add insult to injury."

And so, except for a few victims "lucky" enough (1) to have their injury caused by a product with a provable defect, (2) to be able to hire a very expensive plaintiff's lawyer, and (3) to possess a case which—miracle of miracles—can survive a long course strewn with countless pitfalls, the hundreds of million of dollars spent on product liability insurance largely accrue to a few highly skilled lawyers on both sides, a small group of technical experts, and a number of casualty insurance companies, with the last, of course, loudly complaining about how much money they are losing on the whole operation.

THE situation in regard to medical malpractice cases is depressingly similar, as demonstrated by the recent 412-page report of the National Commission on Medical Malpractice to HEW Secretary Caspar Weinberger. Roughly 18,000 medical malpractice claims are being filed each year, according to this report, with a total cost in court judgments and out-of-pocket settlements of at least $100 million annually. And medical malpractice insurance premiums are soaring. It is not unusual for a doctor to pay from $5,000 to $12,000 *annually*.

But according to a separate Congressional study of medical malpractice, "The lion's share of the total cost to the insurance companies of malpractice suits and claims goes to the legal community." Rick Carlson, an attorney engaged in a study of medical malpractice, writes that "[only] between 16 and 17 cents of the premium

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dollar ends up as benefits to victims of medical injuries," in contrast to 97 cents for Social Security, 93 cents for Blue Cross, 83 cents for much health insurance, and 44 cents for tort liability auto insurance.

Cases of auto accidents, products liability, and professional malpractice constitute the most significant categories of personal injury litigation in our society. (Industrial injuries, the other great category of accidents, were largely removed from courthouse litigation by no-fault workmen's compensation statutes passed by the various states from shortly after the turn of the century on.) Other categories remain, however: falls in stores, hunting accidents, and the myriad of other ways injuries can occur. For these cases, too, the fault (or tort liability) system operates with its heavy hand.

One apparent solution to compensating accident victims is simply to abandon tort liability of any kind and pay everyone injured by accidents for his wage loss and medical expenses under social security or some other form of social insurance—impending national health insurance, for example. Unfortunately, the costs would be staggering. Estimates vary, but the potential costs of national health insurance—covering only medical bills but not any wage loss—are already causing great unease. Even President Nixon's relatively modest new health insurance proposal is estimated by the Administration to entail costs of $40 billion annually, and the Administration claimed that Senator Edward Kennedy's earlier, more ambitious bill would have cost about twice that amount. Daunting figures indeed.

Even if a comprehensive national health insurance program were to be passed in the immediate future, however, it would still meet only a relatively small portion of the total personal injury losses of accident victims. In the case of auto accidents, for example, estimates are that 74 per cent of injury losses is for lost wages, and only 22 per cent for medical costs (with 4 per cent for "other expenses"). Given the high costs that national health insurance is likely to impose, it will be a long time indeed before any such plan or Social Security is extended to cover wage loss as well.

How, then, can the no-fault principle be applied to non-auto accidents? Could one lump all injuries from other than auto or work accidents together under one general statute, creating a new no-fault law of "enterprise liability"? Such a law might provide that every enterprise whose products entail distinctive risks of personal injury (such as a firm that makes ladders or manufactures power tools) must pay for the injuries it generates, without regard to anyone's fault. A man blinded by a rock flung out by a power mower, for example, would be automatically recompensed without endless litigation over whether the mower was defectively constructed or whether he was careless in using it. Under such a law—as under no-fault auto insurance—hugely expensive investigations and litigation over defects and fault would be eliminated. Also, as under no-fault auto insurance, in order to help finance paying for such vital losses as medical expenses and lost wages, no payment would be made for so-called pain and suffering.

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Would this arrangement work for non-auto accidents, as it is obviously working in the case of auto accidents? No-fault auto insurance has been relatively easy to effectuate because of two interrelated factors: 1) an already extant system of widespread fault liability insurance, readily and simply transferable into no-fault loss insurance, without much fear that the transformation would impose new and formidable burdens on anyone; 2) a sufficiently dangerous and well-defined activity (i.e., driving a motor car) that the statute can readily identify who is to be required to pay for what loss.

But with most other accidents—those from medical malpractice or manufactured products, for instance—no-fault insurance poses much greater problems because neither of these two factors is generally applicable. In regard to the first, for example, if ladder manufacturers were to be made liable for falls from ladders, regardless of fault or defect, wouldn't this be likely to expand their liability exponentially? As to the second factor, does the construction of a cement patio constitute an activity sufficiently dangerous that, say, the subcontractor who lays the concrete should be automatically and indefinitely liable to anyone falling on the concrete and injuring himself?

One solution would be to allow any enterprise to *elect* to pay for the injuries it causes on a no-fault basis, thereby foreclosing claims based on fault. The enterprise would be allowed to select all or, if it chose, only a certain number of clearly specified risks of personal injury that its products typically create and agree to pay for out-of-pocket losses when injury results from those risks.

The incentive to substitute no-fault liability for traditional liability based on fault would be that, although the enterprise might have to pay more injury victims, it would pay each victim less: It would not have to pay costs already compensated for by Blue Cross or sick leave, for example, or "costs" for pain and suffering. This would eliminate paying *anything* in most cases of smaller injuries; it would also reduce the amounts to be paid in cases of larger injuries. Moreover, the enterprise would save the huge amounts now spent on legal fees and other expenses in trying to prove that various accidents were not its fault. Another incentive, for manufacturers and doctors, is that the *stigma* of liability would be substantially—and often totally—removed. No longer would the manufacturer or doctor be paying because his product was negligently assembled or because he was found guilty of malpractice, but rather on the morally neutral ground that the accident was just that—an accident.

Under an elective no-fault liability system, a power tool manufacturer could opt for a no-fault policy to pay for out-of-pocket losses—medical expense and wage loss—whenever an amputation resulted from use of the power tool. And just as no-fault auto insurance pays without regard to anyone's negligence, so payment would be made in this instance without regard to the victim's possible care-

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lessness or the lack of any defect in the tool. Similarly, a doctor, knowing the inevitable risks of, say, cardiac arrest even in normal patients regardless of anyone's fault in the course of a given surgical procedure, could agree to pay automatically for out-of-pocket losses stemming from such an adverse result of an operation. In both cases the prior agreement to make such no-fault payments would wipe out any claim based on fault, just as happens under no-fault auto insurance.

Strong precedent for such an elective approach to no-fault accident law exists in states which allow—but do not compel—employers to elect to be covered under no-fault workmen's compensation for employees' injuries, thereby avoiding liability based on fault. In these states, most eligible employers choose to be covered, which would seem to augur well for the prospects of elective no-fault liability for non-employee injuries as well.

By making no-fault liability elective, factors (1) and (2) mentioned above are rendered applicable to many other kinds of accidents besides those involving autos: 1) Manufacturers, doctors, or others may decide for themselves if their fault liability insurance can be readily and simply transferred into no-fault loss insurance without additional burdens to themselves. 2) Similarly, the decision as to who is to be required to pay what is not left open-ended, but is defined by the electing party in his own no-fault liability policy.

Certainly, an astute businessman concerned about skyrocketing premiums under present law would be inclined to check on what no-fault liability would cost him in comparison with regular tort liability. Many enterprises might find the cost much less—especially businesses facing a myriad of regular tort claims of a nuisance nature, such as falls in stores, deleterious matter in food, and so on, where relatively trivial claims far outnumber those for serious injury.

Elective no-fault liability may make especially good sense for the medical profession, as well as for pharmaceutical houses. Unquestionably, some medical procedures entail consistent risks—anesthesia, neurosurgery, orthopedics, and certain side effects from drugs come to mind—and, accordingly, they often lead to medical malpractice claims. For many such procedures, it would make sense to allow health care providers to elect to pay automatically for out-of-pocket losses resulting from them.

In other cases, electing no-fault insurance might not bring significant savings, but it is not inconceivable that humanitarian instincts would nonetheless tip the scale in its favor. When the no-fault auto insurance controversy was reaching its peak, a chief executive of a major car rental company read of the fantastic waste of the auto liability insurance system. Shocked and concerned by how little victims were being paid from auto liability insurance in relation to its cost, he called in his general counsel to investigate the matter further. The result was that his company, along with other car rental firms, became advocates of no-fault auto insurance, even to the point of testifying and lobbying in various legislative

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forums, including the Congress. It is true that, in this instance, projected cost savings under no-fault were also a factor, but the businessman's outrage at the waste and cruelty of the tort liability system was a genuine catalyst.

One may well ask, however, why anyone should be allowed to elect to pay, under no-fault liability, a possibly lesser amount for injuries associated with his product or activity than he might pay under tort liability based on fault. The answer is that no-fault is based on the premise that the regular tort liability is often absurdly and unnecessarily wasteful, dilatory, and cruel. Far better, in this instance, to spend less and spend it wisely. Paying injured parties without the expense and delay of establishing fault or a defect in a product is much more prudent that funneling money through the regular tort liability system.

It will be noted that under elective no-fault liability, as also under no-fault auto insurance and traditionally no-fault workmen's compensation insurance, the law no longer focuses on isolated conduct by individuals as the cause of accidents. Why should that be?

Speaking only of injuries to persons, not property, the basic premise is that in a mechanical and complex age, in which the means for injuring one another abound, unintentional lapses are inevitable, along with their attendant injuries. When those injuries occur, a sophisticated and sensitive society ought to cushion and spread the losses incurred as effectively and efficiently as possible, and indeed with a measure of courtesy and compassion. Given the literally accidental way fault arises in most accidents, exhaustive attempts to analyze and prove individual fault are a fruitless, bootless business. Instead, considerations of how best to help those who are hurt ought to play the dominant part in constructing an insurance mechanism. And encouraging enterprises to pay for all the injuries they generate may also encourage them to find safer modes and procedures in order to reduce their costs.

But if having enterprises bear the burden of personal injury losses encourages them to be more careful, won't relieving individual victims of the burden of their losses encourage them to be more careless? The answer, I think, is no.

A rule which denies payment to injury victims for their losses if they are at fault will never make them more careful, since the desire to avoid being hurt in the first place invariably will be infinitely more compelling than the desire to avoid financial loss. As a practical matter, then, individuals driving cars or using power tools—or deciding to do such things—will never be significantly influenced by fear that careless conduct on their part will forfeit their claim to insurance payment, even in the unlikely event that they fully understand tort laws. (Moreover, only payment from liability insurance is sacrificed by carelessness; health insurance, sick leave, fire insurance —even life insurance to survivors—are all paid regardless of fault.) A rule that denies payment to an accident victim because he was at

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fault will virtually never contribute to making the potential victim more careful, any more than a rule that provides payment to him for his medical bills will encourage him to hurt himself.

As to the effect of a no-fault compensation system on deterrence of unsafe medical practice, one is reminded of a remark by Nathan Glazer (made in comparing the effect of prepaid versus fee-for-service systems of patient care, but equally applicable here): "In all systems, one must depend heavily on the professional ethic of the doctor to provide good and responsible care." Also, under any form of no-fault insurance a number of effective deterrents to accident-producing behavior remain in force. First, criminal sanctions remain for egregiously negligent behavior. Second, administrative sanctions remain, such as the loss for a doctor of a medical license or hospital privileges. Third, for any enterprise, insurance premiums can be varied according to its safety record, as has long been done under no-fault workmen's compensation.

In discussing the advantages of no-fault auto insurance, Moynihan has stated that the proponents of no-fault "are right in the all-important perception as to what it is Americans are good at. We are good at maintaining business relationships once a basis of mutual self-interest is established. [No-fault auto insurance] . . . would establish one." Elective no-fault liability, by allowing the businessman and his customer or a doctor and his patient to bypass the lawyers, with all their incredibly inappropriate and self-serving cumbersomeness, is also surely an excellent example of establishing a relationship based on mutual self-interest.

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CURRENT READING

More on Methadone

In the first article of this issue, Edward Jay Epstein looks at some of the disappointed hopes and unintended consequences of methadone maintenance. A striking corroboration of his analysis may be found in a recent study by the Drug Enforcement Agency (DEA) entitled "Methadone: A Review of Current Information." (Although dated December 1973, this study was not released until April 1974, too late for inclusion in Mr. Epstein's article.) The DEA report begins with a summary of its key conclusions:

> Methadone abuse is rapidly rising. Increasing methadone deaths, declining heroin deaths, and a 5-1 predominance of methadone to heroine deaths in the last six months suggest that methadone is partially replacing heroin as a drug of abuse in certain areas of the U.S.
>
> Methadone addicts are not necessarily *rehabilitated* heroin addicts. A limited study of heroin and methadone addicts indicates that methadone addicts are equally prone to arrest, are more prone to commit property crime or crimes of assault, and are usually unemployed; almost half of those arrested obtained methadone outside of treatment programs.
>
> By the last quarter of the reporting period, the combined total of drugs acting alone did not cause as many deaths as poly-drug use, which accounted for 54% of all deaths reported from April through June 1973. This suggests that poly-drug abuse may have become the major drug problem in the U.S. Proportionately, methadone had the highest percentage of poly-drug deaths (66% of methadone deaths reported in the final quarter) with barbiturates second (62%). . . .

The study's findings on crime and unemployment among methadone users, based on addict arrest sheets in New York, Chicago, and Detroit during the summer of 1973, are detailed in a later section:

> . . . [A]lmost half of those addicted to heroin were involved in drug related crime with only one-third involved in property crimes or assaults. By contrast, 51% of methadone arrests were for property crimes or assault and only 17% for drug crimes. This would suggest that methadone does not necessarily alter the socio-economic crime related syndrome or, in fact, lower the crime rate. . . .

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. . . [I]n New York City, where there is an extensive methadone program, there were more methadone addicts arrested for various crimes than heroin addicts.

Although employment was slightly higher among methadone addicts, it is more significant that a total of 70% of methadone addicts were unemployed and that there was little difference between the employment rates of heroin addicts and those addicted to methadone. In short, it appears that both heroin addicts and methadone addicts depended equally on either crime or welfare programs for income, and that methadone maintenance does not necessarily insure that heroin addicts will become contributing members of society when they enter methadone maintenance. . . .

Although the data is quite limited, the statistics concerning addict arrestees bring into question the theory that methadone maintenance rehabilitates heroin addicts, takes them out of the crime syndrome, and gives them incentive to contribute to society via employment. Also, the data on addicts not in treatment suggests that methadone maintenance programs have had the side effect of making methadone more available as an illicit drug of choice.

The Best-educated Generation?

What kind of books do today's college students prefer to read? A fascinating answer to this question is provided by *The Chronicle of Higher Education*'s list (published April 15, 1974) of the 12 best-sellers on campus in 1973:

1. *I'm OK, You're OK: A Practical Guide to Transactional Analysis,* by Thomas A. Harris
2. *Jonathan Livingston Seagull,* by Richard Bach
3. *Chariots of the Gods?* by Erich von Daniken
4. *Journey to Ixtlan,* by Carlos Castaneda
5. *The Teachings of Don Juan,* by Carlos Castaneda
6. *A Separate Reality,* by Carlos Castaneda
7. *Our Bodies, Our Selves,* by Boston Women's Health Book Collective
8. *Open Marriage,* by Nena and George O'Neill
9. *Beyond Freedom and Dignity,* by B. F. Skinner
10. *The Best and the Brightest,* by David Halberstam
11. *The Joy of Sex,* by Alex Comfort
12. *Gods from Outer Space,* by Erich von Daniken

The Cash Nexus Humanizes!

Once upon a time Karl Marx and Friedrich Engels railed against bourgeois society for leaving "no other nexus between man and man . . . than callous 'cash payment.'" Now, however, some of our contemporary "humanists" have chosen resolutely to defend the cash nexus against the sinister forces that threaten it. A recent article in *The New York Times* tells the story:

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Over the protest of a handful of humanists, plastic credit cards have been issued to members of the Cornell University faculty club. Cash is no longer accepted for meals.

Until a fortnight ago, one holdout insisted on paying in legal tender, even in the face of a 10 per cent penalty for cash. She was Mrs. Rose K. Goldsen, an associate professor of sociology.

"At this point in American history," she wrote to The Cornell Chronicle, "any measure that reduces face-to-face, person-to-person interaction and replaces it with person-to-machine interaction, erodes further whatever sense of community some of us are trying, against odds, to keep alive on the campus." . . .

The Future of New York City: I

There are many who view with alarm the increasing concentration of minority-group populations in our major cities, believing that this trend threatens to engulf the cities in a sea of poverty. But in the Spring 1974 issue of *New York Affairs,* Peter D. Salins offers a much more optimistic evaluation of the demographic trends—at least for New York City. His intriguing predictions about New York's future suggest that a vast increase in minority populations need not mean a decline in prosperity:

The New York State portion of the Standard Metropolitan Statistical Area will continue to grow in population from its present level of eleven million to over fourteen million by the year 2000. However the entire growth will be accounted for by increasing numbers of blacks and Puerto Ricans, with the metropolitan non-minority population declining slightly from 8.8 million today to 8.2 million at the end of the century. The city, on the other hand, will remain virtually static at its present population of just under eight million—experiencing, if anything, a slight decline over the next three decades; but its racial composition will continue to shift significantly. There will be an unrelenting out-migration of non-minority families, their places being taken by blacks and Puerto Ricans until minority families comprise half the city's population by 1990 and become a clear majority of nearly 60 percent by 2000. Even with most of the metropolitan growth of the black and Puerto Rican population being absorbed by New York City, a large part of the increase will be found in the inner suburban counties of Nassau and Westchester. The already largely mythic concept of the totally white suburban ring will be a total fiction within the next two decades. . . .

The steady but gradual change in the population trends of the city as a whole will have more dramatic consequences in the individual boroughs. Manhattan will continue to lose population (it has already dropped from nearly two million in 1950 to barely more than a million and a half in 1970) at the rate of better than 100,000 persons a decade. But it will be losing families selectively. It will see the departure of many of its present black and Puerto Rican families, and its other ethnic remnants in Little Italy, Chinatown, the Lower East Side, York-

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ville and Washington Heights. The population remaining will increasingly consist of the quintessential Manhattanites that already dominate both the sedate and the "swinging" East Side, the Upper West Side, Greenwich Village, Murray Hill and Gramercy Park. This composite type is characterized neither by its elevated class nor its high income, but its willingness to pay large rents for little space.

The Bronx will be the only one of the "big four" boroughs to gain population continuously over the next three decades, growing from nearly 1.5 million persons today to over 1.6 million by 2000. This overall increase will occur only because of an unparalleled growth of the black and Puerto Rican population large enough to offset the nearly total exodus of non-minority families. "Minority" families already account for nearly half the borough's population and will constitute an unprecedented 90 to 94 percent by century's end. . . .

The second largest concentration of minority families will be in Brooklyn. Although its total population is expected to continue to decline in the next three decades, its black and Puerto Rican proportion will rise from 36 percent of the total to over 72 percent by the year 2000. A growing middle-class black population will continue to move southward from the central Brooklyn ghetto to the Rockaway Inlet waterfront until it dominates or comprises a large minority of such neighborhoods along the way as East Flatbush, Flatbush, Midwood, Brighton Beach, Canarsie, Sheepshead Bay, Mill Basin and Bergen Beach. Less affluent Puerto Rican families to the west will be moving south from present Puerto Rican strongholds in South Brooklyn and the Gowanus Canal area into Borough Park, Bensonhurst and Bay Ridge. As a rough generalization, we might say that the blacks will gradually come to dominate the Jewish and Irish neighborhoods of Brooklyn while Puerto Ricans will settle in the neighborhoods that are now largely Italian. . . .

Demographically, as in other respects, Queens will be the most stable of the city's boroughs. The total population will remain at or near its present level of slightly under two million for the remainder of the century. While its minority population will continue to grow from 15 percent of the total today to over a third by the year 2000, this will represent the least noticeable or significant shift in any of the outer boroughs excluding Staten Island. Aside from the smaller numbers involved, this will be the case because the black families of Queens will continue to be, as they are today, just as well-off economically, occupationally and educationally, as their white counterparts in the borough. . . .

Staten Island will be the city's fastest growing borough in terms of its rate of population increase, swelling from its present 295,000 persons to over 700,000 by the year 2000. At century's end, with two-thirds of Manhattan's population, Staten Island will no longer be the city's insignificant kid brother. The increase in the borough's population will consist overwhelmingly of non-minority families. As a result Staten Island will remain not only the whitest of the city's boroughs but it will probably be whiter than much of suburbia. . . .

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The shift in New York's racial composition does not mean that the city will be becoming less middle-class or even significantly poorer. As rapidly as it loses white middle-class families to the suburbs, New York will be recruiting new middle-class families from among the increasingly affluent ranks of its swelling black and Puerto Rican population. With the gradual ending of the in-migration of poor families from Puerto Rico and the South, there will be no offset to the upward mobility of today's minority members and their children. . . .

While the overall class composition of the city will remain stable over the next three decades—even improving slightly—with roughly two-thirds of New York's families being middle-class or better and a sixth classifiable as poor, the class composition of the boroughs will vary widely.

Manhattan will become, if anything, even more an enclave of what the rest of the city will *consider* upper middle class (limousine liberals, "West Side intellectuals," "silk stockingites" and so on). Queens and Staten Island will remain bastions of the non-minority middle class, although Queens will also contain a growing cadre of middle-class black families even more affluent than its whites. Brooklyn, remaining a borough of pleasant houses on tree-shaded streets, will eventually become the major settlement area for upwardly mobile black and Puerto Rican families. The Bronx, on the other hand, will be severely divided between a large middle-class, Puerto Rican and black population northeast of Van Cortlandt Park, and the most horrendous poverty of the metropolitan area in the southwest. . . .

For the foreseeable future about one sixth of the city's population will be quite poor. At the very least, regardless of their actual income, families in the lower sixth of the income distribution will be considered poor, will contribute disproportionately to the incidence of crime and other "deviant" behavior, and will live in the most unsatisfactory physical and social environments, which will continue to be called "slums." Although a growing majority of black and Puerto Rican families will *not* be poor and will not live in slums, the overwhelming majority of slum dwellers will continue to be blacks and Puerto Ricans. . . .

The Future of New York City: II

Before our readers take too much encouragement from the previous item, we must report that a second article—by Dick Netzer—in the same issue of *New York Affairs* paints a much gloomier picture of New York City's future. Netzer derives his more sobering projections of the city's economic prospects principally from employment and business indicators, rather than the demographic data relied upon by Salins:

. . . [I]n 1960, a massive and sober reexamination of the prospects for the economy of the New York metropolitan region had been completed, and its projections for the city's economy were generally favorable ones. This was the New York Metropolitan Region study, conducted for the Regional Plan Association by a team organized by Harvard's

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Graduate School of Public Administration under the direction of Raymond Vernon, and financed by a consortium of foundations. The results of the study were published in a series of volumes between 1959 and 1961; the summary volume was Vernon's *Metropolis 1985* (Harvard University Press, 1960).

The Vernon projections were based upon a distinctive view of the historic processes of economic development in the New York region, as well as intensive analyses of the state of key economic activities as of the mid-1950's. Vernon saw, as New York's historic economic genius, its ability to pioneer in the development of a set of specialized economic activities and to replace those activities with new specialties as the older ones shifted to other parts of the country, in quest of growth markets elsewhere, lower wage rates, or whatever. That is, he saw the region in a process of continuous economic rejuvenation, losing out competitively with respect to things that had once been distinctly New York-oriented but finding new sources of economic strength for which location in New York is especially advantageous.

Thus, the best harbor on the East Coast combined with the Erie Canal made New York the dominant port in the country early on, generating a variety of port-related activities like marine insurance and wholesale trade in which New York also became dominant. The development of other ports and the advent of the railroad gradually diminished this advantage, but dominance in other kinds of financial activities and some forms of manufacturing became New York's new specialties. In manufacturing, and to an increasing extent in other activities as well, New York specialized in the development of new products, the production of which tended to shift away from New York if and when they became standardized; for New York's advantages lay in the availability of rented space, suppliers and other supporting services, a diversity of labor force skills and large numbers of individual entrepreneurs, all of them important to new, non-standard and risky products. . . .

As of 1965, the Vernon projections appeared to be generally reasonable ones. Total employment was slightly above the projection, as was employment in the "service-exporting" sector, that is, central offices, finance and related services. Employment in manufacturing and wholesale trade had declined fairly sharply, by 140,000 jobs, instead of increasing modestly as the projection indicated (and then remaining relatively flat out to 1985). Government employment had increased significantly, by about 75,000 jobs, while the "all other" category had increased moderately, instead of declining, largely because of the strength in construction explained by the apartment house boom of the early 1960's and the developing office building boom. It seemed reasonable to conclude, as I did in an article reviewing this evidence in early 1969, that where Vernon had erred—aside from not forecasting the rise in public employment—was in underestimating the rapidity of the transition he had anticipated, from a goods-handling to a service-producing economy.

However, his longer-term projections now seem more problematic, in

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the light of events since 1965. Currently, total employment is nearly 200,000 less than an interpolation of his projections would suggest, with goods-handling jobs (mainly, manufacturing and wholesale trade) at least 400,000 below his projection and the deficit in jobs only partly made up by a continued rapid increase in public employment and an even more rapid increase in the service-exporting sector than he had foreseen. . . . The long-range (to 1985) projection envisaged relatively little change in employment in other sectors and a large one in the service-exporting sector, resulting in an increase in employment over thirty years of more than 500,000. What now seems clear is that the high-income professional and managerial economy, with ample employment opportunities for all, that he foresaw, can be realized only if government and office employment continues to increase at rates far more rapid than Vernon projected, while the rate of decline in the goods-handling sector slows. . . .

Most goods-handling activities exhibited job declines during the decade [1960-70], with exceptionally large relative declines in New York's number one manufacturing industry, the garment industry, printing and publishing (other than book publishing) and freight transportation. One disturbing characteristic of this decline was that it was *not* offset by large increases in such jobs in other parts of the New York region, which had been the tendency earlier. During the boom years in the national economy, between 1961 and 1969, manufacturing employment increased by about one-fourth nationwide; it increased in the region outside New York City—at far less than the national rate—and declined in New York City. So a fair number of jobs lost to New York City were lost to the region as well. Moreover, some of the largest gains in the region were relatively remote from the city, in the more distant parts of the New Jersey sector of the region, like Middlesex and Somerset Counties. Thus, the job loss in the city did not represent jobs slipping over the city line into Nassau or Yonkers, or just over the Hudson River, where they would be accessible to reverse commuters who live in the city.

A second disturbing feature is the pervasive nature of the declines in manufacturing employment in the city during the national upswing between 1961 and 1969. The Vernon model predicts that new specialties, in manufacturing as in other industries, arise to replace those for which New York is no longer a good location, with New York entrepreneurs capitalizing on new tastes, processes and technology to develop these replacements. But between 1961 and 1969, employment declined in a large majority of all types of manufacturing industry. There were distinct increases in employment in only a half-dozen smaller industries (which together account for only about one-twelfth of all manufacturing jobs), including jewelry, tobacco products, knitting mills, plastic products, office and art supplies, and book publishing. Further, the last two of these industries are really office activities or closely related to such activities; indeed, all the "growth industries" probably had their growth in the form of jobs in offices, showrooms and other distribution activities, or repackaging of large shipments into

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smaller consignments, jobs that are very unlike the usual image of true (and well-paid) manufacturing employment. . . .

Yet another disquieting observation relates to New York City's traditional role as a "hothouse" or incubator for new manufacturing activities. Throughout the country, there is considerable visible evidence that suburban locations can perform very well indeed as incubators for new firms making new products; this is the case for Route 128 in the Boston area as it is for Jericho Turnpike in Nassau County. The entrepreneurs who live in the suburbs now seem able to operate their businesses in the suburbs as well. Statistical testing of this is difficult, but there is one piece of confirming evidence. In the last decade, New York City lost relatively more manufacturing and wholesaling *enterprises* than jobs. Thus, the average size of firm rose significantly. It is still well below the national average for manufacturing enterprises, but the trend is bad news for the incubator-industry model of job growth in the city. . . .

However bad the situation of goods-handling activities in the city, the increasing service orientation of the city's economy has yielded an increase in the number of good jobs, that is, jobs in the higher echelons in the standard classification of occupations—professionals, technical workers, managers and administrators. In 1970, roughly 26 percent of the employed residents of the region held jobs of that type, and about the same proportion of all jobs located in New York City (without regard to the *residence* of the job-holder) were professional and managerial jobs, which is well above the nationwide average. On the average, professional and managerial jobs yield earnings that are nearly twice those in other occupations. Between 1960 and 1970, such jobs apparently accounted for 125,000 of the total increase of roughly 200,000 in employment in the city (most of the increase in the total has been lost by now, as was noted earlier).

That would seem to imply a significant improvement in economic status over the decade. However, *most of the improvement did not accrue to city residents* (emphasis added). I estimate that city residents received only about 50,000 of the 125,000 new professional and managerial jobs and commuters 75,000. Thus commuters received an entirely disproportionate share of the new well-paid jobs, since in 1960 they had accounted for less than 30 percent of the professional and managerial jobs in the city. Of course, this process did not entail the transfer of good jobs from people living in the city in 1960 to people living in the suburbs in that year; rather, it was a consequence of heavy residential migration to the suburbs on the part of professional and managerial types.

Not surprisingly, the result of all this is an increasing disparity between the incomes of city residents and residents of the rest of the region. In 1969, suburbanites employed in the city had median earnings that were 187 percent of those of employed city residents—$16,500 compared to roughly $8,800. The fact is that the city's population as a whole is by now a relatively low-income one, using almost any standard of comparison. . . .

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To our readers

For four years now, THE PUBLIC INTEREST has maintained its current price. This is simply no longer feasible, for reasons we assume to be obvious to our readers. We regretfully announce, therefore, that the following price increases will take effect with our Fall 1974 issue:

Single issue:	$2.50
One year subscription:	$9.50
Two year subscription:	$18.00

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